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R. L. GOODSTEIN

WITH THE ASSISTANCE OF

H. M. CUNDY K. M. SOWDEN

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ALMOST REGULAR POLYHEDRA

BY H. G. APSIMON

I. *Introduction*

In this note I classify certain polyhedra which are of interest in that they have in some sense a higher degree of regularity than any other polyhedra except those which are completely regular. The definition* of a regular polyhedron is that it shall possess two particular symmetries ; one which cyclically permutes the vertices of any face c, and one which cyclically permutes the faces that meet at a vertex C, C being a vertex of c. In the case of three-dimensional polyhedra this definition may be replaced by one which lends itself more readily to relaxation :

A polyhedron is said to be regular if it has equal regular faces and equal regular vertex figures ; (A)

but it should be noted that this second form is not equivalent to the first in space of more than three dimensions, and in § 5 I give examples of polyhedra in space of $2n-1$ dimensions ($n \geq 4$, $\neq 6$) which obey the criteria in (A) but which are not regular ; in § 6 I describe an 11-dimensional regular polyhedron derived in the same manner as those in § 5.

If we relax the conditions in (A) to some degree we obtain wider classes of polyhedra, not necessarily regular ; if, for instance, the conditions of equality of the faces and regularity of the vertex figures are removed, we obtain the Archimedean polyhedra (though it is now necessary to introduce some convention excluding compounds, which may be done by assuming that a polyhedron possesses the property that each face is accessible to any other by paths crossing from one face to another by the edge common to both ; this convention will be assumed throughout this note). But the relaxation of (A) necessary to admit Archimedean polyhedra destroys two of the four conditions implicit in (A), and this suggests that it is reasonable to look for polyhedra obeying conditions more stringent, in that they satisfy some three of the conditions in (A) ; such polyhedra exist, and may be called Almost-Regular. I now examine in turn the four types of such polyhedra which seem *a priori* possible.

* See, for example, H. S. M. Coxeter, " Regular Skew Polyhedra in Three and Four Dimensions ", *Proc. London Math. Soc.* (2) 43 (1937), 33–62.

2. If from (A) we remove just the condition for equality of the faces—that is, if we consider polyhedra having regular faces and equal regular vertex figures—it is seen that the joins of alternate vertices of faces are all equal, since these joins are the sides of the equal regular vertex figures. But, since the faces are regular, this implies that they are also equal, and so no new class of polyhedra is obtained by the proposed relaxation.

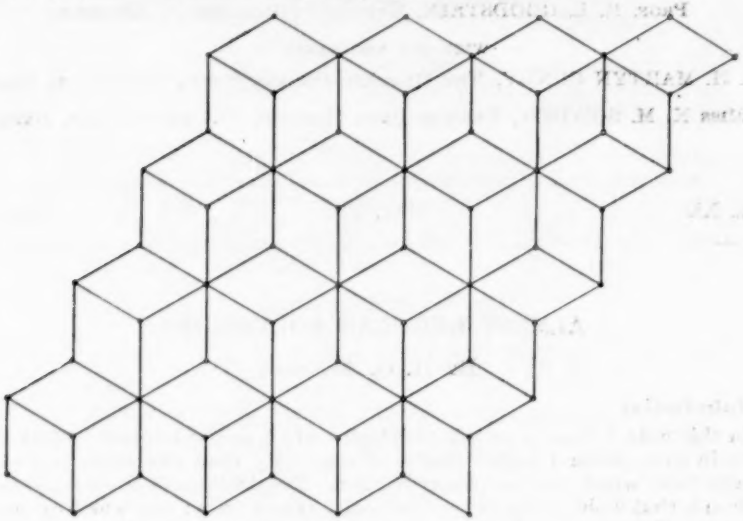

FIG. 1.

3. If from (A) we remove the condition for regularity of the vertex figures we obtain the Facially-Regular polyhedra, while if from (A) we remove the condition for regularity of the faces we obtain the Vertex-Regular polyhedra—examples of which I have given in two previous notes.* I should like to add here a conjecture concerning these; the facially-regular polyhedra fall into two types; (*a*) having their faces similarly surrounded, in that a symmetry of the polyhedron exists that can transform any face into any other face, and (*b*) not having their faces similarly surrounded. The only known examples of type (*a*)—other than, of course, regular polyhedra—may be symbolised by 3^9 and 3^{12} (i.e. they have triangular faces, nine and twelve faces meeting at each vertex respectively), and the only known vertex-regular polyhedra are in a sense the reciprocals of these; having nine or twelve sided faces (three meeting at each vertex) whose centres are the vertices of the corresponding facially-regular polyhedra. It would seem probable that any vertex-regular polyhedron is the reciprocal in this sense of a facially-regular polyhedron of type (*a*).

4. The final possible relaxation of (A) is to remove the condition for the equality of the vertex figures; this gives polyhedra defined as having equal regular faces and regular vertex figures. As with the facially-regular polyhedra, such polyhedra fall into two types: (*a*) having their faces similarly surrounded, and (*b*) not having their faces similarly surrounded. The only example I can find (Fig. 1) of type (*a*) may be symbolised as $4^{3.6}$—that is, it has square faces and either three or six faces meeting at each vertex—and

* "Three Facially-Regular Polyhedra", *Canadian Journ. of Math.* 2 (1950) 326–330; "Two Vertex-Regular Polyhedra", *ibid*, 3 (1951) 269–271.

may be represented in rectangular Cartesian coordinates as having vertices at the points

$$(\xi, \xi+\eta, \eta) \quad \text{(6-vertices)}, \qquad (\xi \pm 1, \xi+\eta, \eta) \quad \text{(3-vertices)}$$

where ξ and η take all integer values independently; it has as its vertex figures either equilateral triangles or regular skew hexagons (whose sides are the sloping edges of triangular antiprisms).

Mr. S. Melmore has pointed out to me as an example of type (*b*) a finite polyhedron, also having the symbol $4^3, 6$, with thirty faces; it may be considered as being comprised of the visible faces of a piling of six cubes one onto each of the faces of a seventh (Fig. 2).

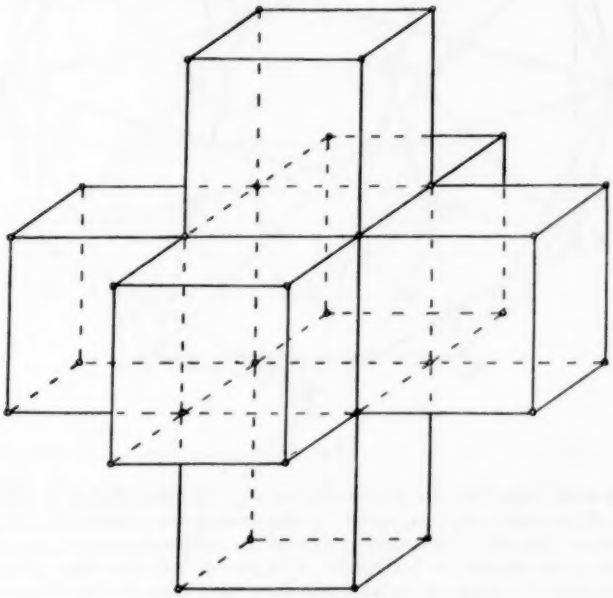

FIG. 2.

5. It was stated in § 1 that the definition (A) was insufficient to ensure regularity in space of more than three dimensions; I illustrate this with a series of multidimensional polyhedra which satisfy (A) but which are not regular.

Consider the simplex α_{2n-1} $(n \geqslant 4)$, and label its vertices $A_r (r = 1, 2, \ldots, 2n)$; select from the equilateral triangles formed by triads of these points those having vertices

$$A_r \quad A_{r+1} \quad A_{r+2},$$
$$A_r \quad A_{n+r+1} \quad A_{r+2};$$

with the convention that $A_{2n+r} \equiv A_r$. The polyhedron P_{2n} (see, for example, Fig. 3, where $n = 6$) formed by these $4n$ triangles has no free edges, and the vertex figure of any vertex A_r is the skew hexagon formed by the vertices

$$A_{r-2} \quad A_{r-1} \quad A_{r+1} \quad A_{r+2} \quad A_{r+n+1} \quad A_{r+n-1}$$

in order; this vertex figure has for its vertices the vertices of an α_5, and so is regular. Hence P_{2n} has equal regular faces and equal regular vertex figures, and so satisfies the criteria of (A).

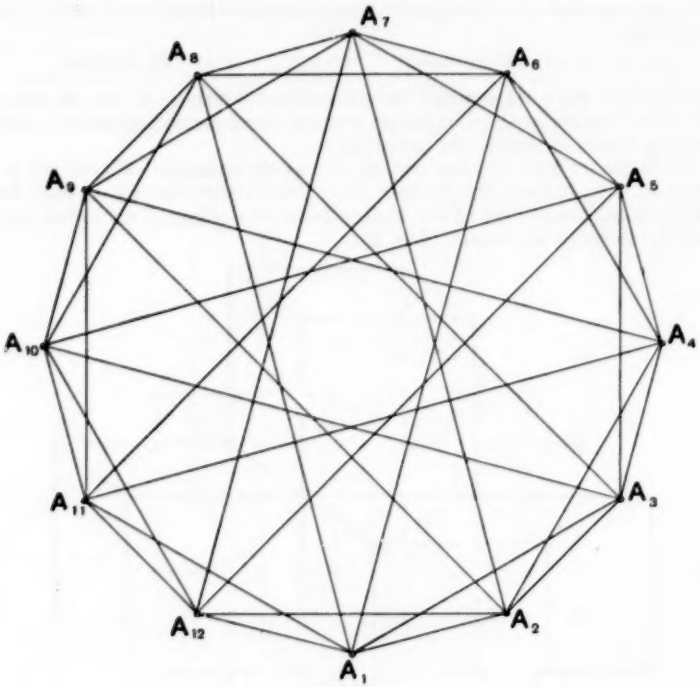

FIG. 3.

We will now examine the regularity of P_{2n} by identifying it with a map* of type {3, 6}, which is representable by the triangular tessellation {3, 6}, whose vertices have integer coordinates referred to oblique axes (x, y) inclined at $\pi/3$. Such a map is said to be regular if its group includes the cyclic permutation of the edges belonging to any one face, and also the cyclic permutation of the edges that meet at any one vertex of this face ; and Coxeter has shown that such a map (and consequently the corresponding polyhedron) is regular if and only if all points of the lattice generated by the vectors (b, c), $(-c, b+c)$ are identifiable, and that such a map will have b^2+bc+c^2 vertices. Then since P_{2n} has $2n$ vertices we need only consider cases in which

$$b^2+bc+c^2=2n\ ;$$

as a first requirement b and c must be even. It is seen that, by a suitable choice of axes (see Fig. 4), the map point obtained by a translation b along the x axis from the point A_r is A_{r+2b}, and that obtained by a translation c along the y axis from the point A_r is A_{r+c} if c is doubly even and $A_{r+c+\frac{1}{2}nc}$ if c is singly even ; and therefore that the map point obtained by the translation (b, c) from the point A_r is A_{r+2b+c} if c is doubly even and $A_{r+2b+c+\frac{1}{2}nc}$ if c is singly even.

Take first the case in which c is doubly even. It is then a necessary condition for regularity that A_{r+2b+c} must be identifiable with A_r, and so $(2b+c)$ must be an integer multiple of $2n$, i.e. of b^2+bc+c^2, which is impossible (unless $b=c=0$, which is trivial). Hence a regular map is impossible.

* For the definitionof this and other terms of this section see Coxeter, " Self-Dual Configurations and Regular Graphs ", *Bulletin of American Math. Soc.* 56 (1950)

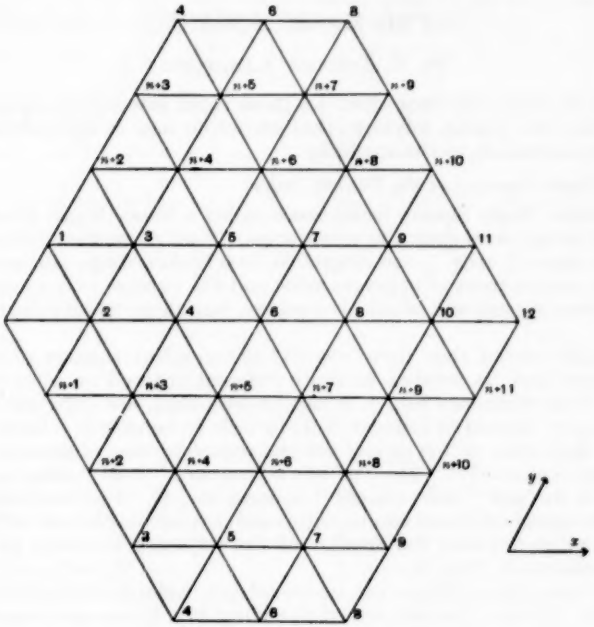

FIG. 4.

Take now the case in which c is singly even. It is then a necessary condition for regularity that $A_{r+2b+c+\frac{1}{2}nc}$ must be identifiable with A_r, and so $(2b+c+\frac{1}{2}nc)$ must be an integer multiple of $2n$; i.e. $2b+c+\frac{1}{2}c(b^2+bc+c^2)$ must be an integer multiple of b^2+bc+c^2. Writing for convenience $b=2B$, $c=2(2C+1)$, we must be able to solve in non-negative integers

$$2B+2C+1=(2k-2C-1)\{B^2+2BC+4C^2+B+4C+1\},$$

which is impossible unless $C=0$, $B=0$ or $C=0$, $B=1$. Hence the only possibility which need be considered further is $b=c=2$ (when $c=2$, $b=0$ we have $n=2$, which is below the range of n in which we are interested).

We have then shown that the polyhedron $P_{2n}(n\geqslant 4,\ \neq 6)$ satisfies the criteria of (A) but is not regular; the case $n=6$ requires further investigation.

6. *The Regular Polyhedron* P_{12}

Taking the particular case $n=6$ of the series of polyhedra P_{2n} defined in § 5, we map P_{12} (which has b^2+bc+c^2 vertices, where $b=c=2$) (Fig. 3) into the plane tessellation $\{3, 6\}$, and obtain a map in which the map point obtained by the translation (b, c) from A_r is $A_{r+2b+c+\frac{1}{2}cn}=A_{r+12}\equiv A_r$, and the map point obtained by the translation $(-c, b+c)$ from A_r is $A_{r-2c+b+\frac{1}{2}bn+c+\frac{1}{2}cn}=A_{r+12}\equiv A_r$, and so lattice points generated by the vectors (b, c), $(-c, b+c)$ are identifiable. Hence this map $\{3, 6\}_{2,2}$ is regular, and so P_{12} is regular.

Summing up, there exists an 11-dimensional finite regular polyhedron having 12 vertices, 24 (triangular) faces, and 36 edges, whose vertices are those of an α_{11} and which is identifiable with the regular map $\{3, 6\}_{2,2}$.

Decca Research Laboratories H. G. APS.

SUPER MAGIC SQUARES

By C. Dudley Langford

Though much work has been done on these most interesting squares, there appear to be a few points, anyhow, that are either new or not published anywhere easily accessible to the majority.

Algebraic Magic Squares of the Fourth Order

An Algebraic Magic Square (sometimes called a Super Magic Square) adds up to 34 in many more than the usual ways. Four rows, four columns, each of the four corner " cells ", two diagonals, two broken diagonals, centre four, the corners, centre pairs of opposite sides and the corners of a square 3 by 3. Any particular square will often give others, but these twenty-four apply to all.

It is usually stated that there are 432 independent squares of this type, each of which can be written in eight different rotated or reflected forms. However, if one examines Fig. 1, it can be seen that, not only can four such squares (usually classed as independent) be seen to be closely related, but also the broken diagonals of one square are the complete ones of another, and the other " more awkward " " 34s " of one square are " neat " ones in another. This reduces the 432 " independent " squares to 108. Furthermore, each of these 108 independent forms can be expressed in a standard form with 1 at the top left hand corner and the smaller of the adjacent numbers in the next position horizontally (Fig. 2).

Actually even these 108 are not as completely independent as they appear at first sight. If one takes *any* one of them, say Fig. 2, one can transform it in several ways. One can leave one side as it is, reverse the opposite and change the connecting sides into diagonals and vice versa, to give Fig. 3. Repeating this at right angles gives Fig. 4. A further repetition of the process gets us

8	13	1	12	8	13
2	11	7	14	2	11
15	6	10	3	15	6
9	4	16	5	9	4
8	13	1	12	8	13
2	11	7	14	2	11

Fig. 1

1	7	10	16
12	14	3	5
8	2	15	9
13	11	6	4

Fig. 2

1	7	10	16
14	12	5	3
15	9	8	2
4	6	11	13

Fig. 3

1	12	8	13
14	7	11	2
15	6	10	3
4	9	5	16

Fig. 4

1	7	12	14
10	16	3	5
8	2	13	11
15	9	6	4

Fig. 5

back to Fig. 2 in a new position. In my own notes, I refer to this as the side-diagonal transformation ; it links up the 108 into 36 triplets.

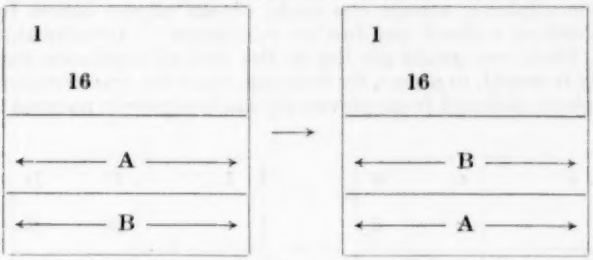

FIG. 6

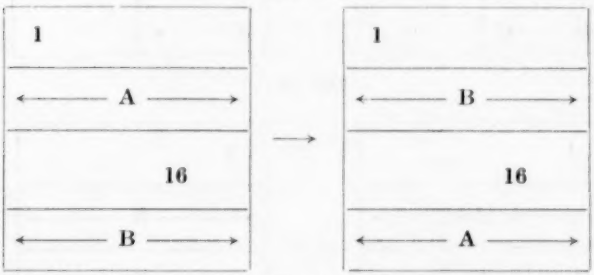

FIG. 7A

Another transformation that can be applied to all algebraic squares is what I called the *oblique shuffle*. Fig. 2 gives Fig. 5. Numbers that have been moved are in bold. This oblique shuffle can also be done at right angles, and again, but if repeated once more gives us the original square. Thus this transformation, by itself, reduces the 108 forms to 27 quartets.

If, however, one uses these transformations one after the other, one can start with but one algebraic square and end, not with just 12, but with a set of 36. So, in a way, we have now reduced the 108 into three independent sets of 36. In the first of these sets, α, neither 7 nor 10 is found in the same row, column, diagonal, etc., as the 1.

In the second set, β, neither 6 nor 11 is in this position. In the third set, γ, it is 4 and 13 that are thus limited. In *no* set can 2, 3, 5 or 9 be in the same row, column, diagonal, etc. as the 1.

Apart from these transformations that can be applied to any algebraic square to give another, there are some that can be applied to special cases, depending on the position of complementary pairs (that is, pairs adding up to 17). If one uses a " standard form " with 1 in the top left-hand corner, this simplifies down to the position of the 16. I hope that Fig. 6 to Fig. 12b will make these clear. 1 and 16 are always shown, unmarked spaces are unchanged, lettered are changed as shown, the letters are used to cover as big blocks as possible ; whenever the 1 and 16 are on the same diagonal, each process can be done as shown or at right angles or alternately. Fig. 8b is not a mirror image about the diagonal ; two quadrants are unchanged and the other two change places *en bloc*. Figs. 12a, 12b involve a change in the position of the 16. Some of the transformations are more interesting when done on an extended number lattice similar to Fig. 1 (for example Fig. 11b, where it then becomes clear that *CE* and *DF* are really adjacent pairs which change places just like the pairs represented by *A* and *B*.

Some of these transformations will enable one to get from one of the main groups of 36 (called α, β and γ above) to another—so, theoretically, starting with any one algebraic square one could obtain *all* the others by a mere process of shuffling without any further calculation ! Actually this has two drawbacks. First, one would get lost in the host of duplicates that appear, and secondly it would, in a way, be cheating, since the transformations were, in the first place, deduced from previously-made algebraic squares !

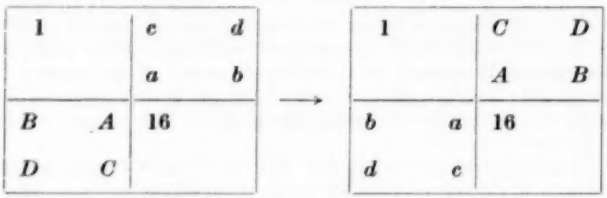

FIG. 7B

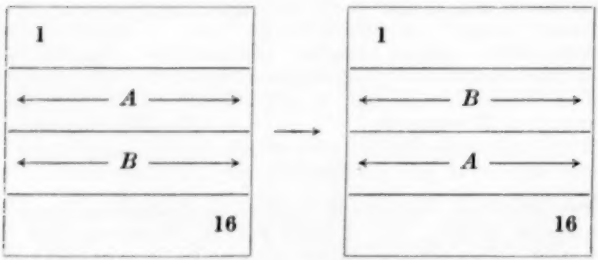

FIG. 8A

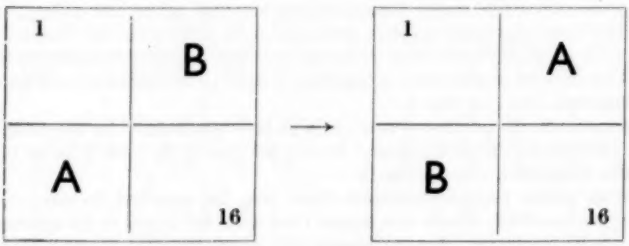

FIG. 8B

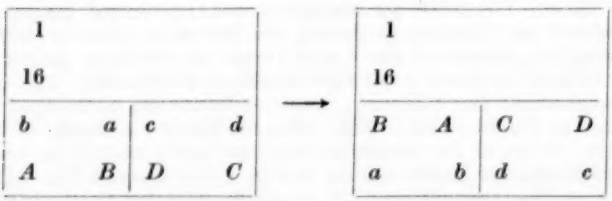

FIG. 9A

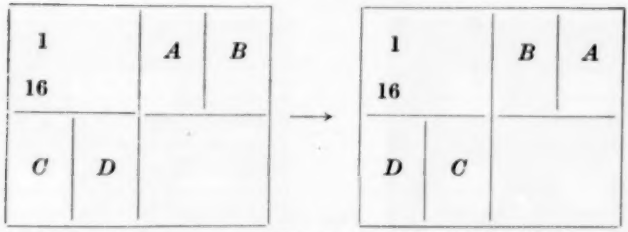

FIG. 9B

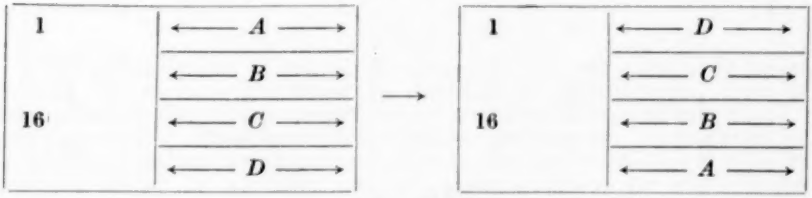

FIG. 10A

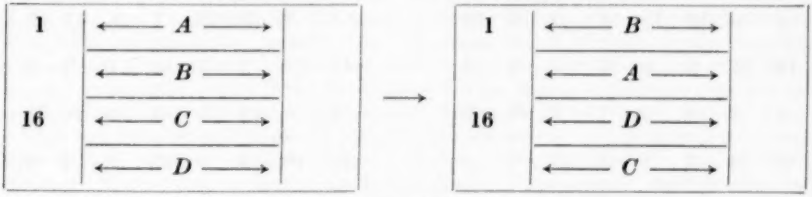

FIG. 10B

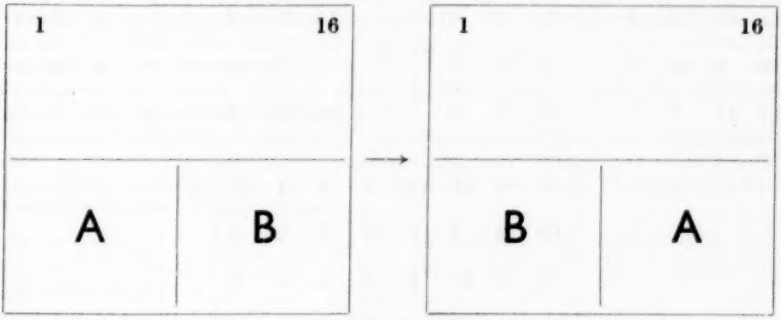

FIG. 11A

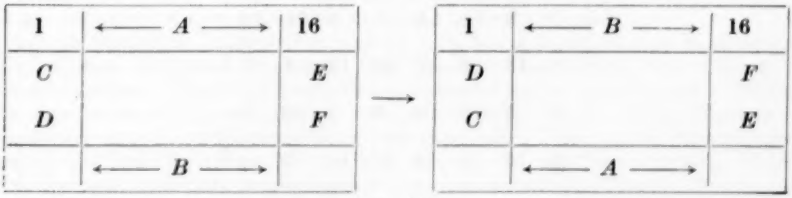

FIG. 11B

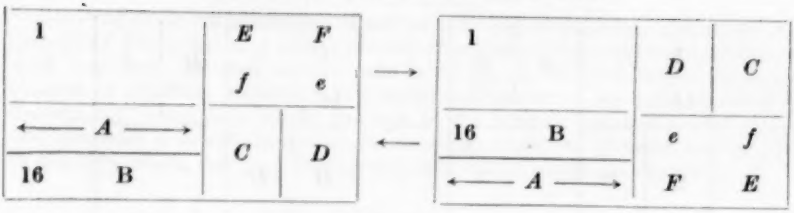

FIG. 12A

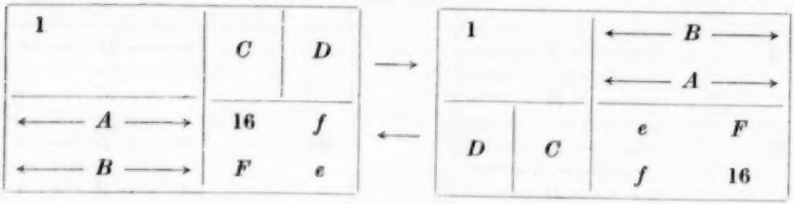

FIG. 12B

1	8	10	15	1	8	10	15
14	11	5	4	14	11	5	4
7	2	16	9	7	2	16	9
12	13	3	6	12	13	3	6
1	8	10	15	1	8	10	15
14	11	5	4	14	11	5	4
7	2	16	9	7	2	16	9
12	13	3	6	12	13	3	6

1	8	13	12	1	8	13	12
14	11	2	7	14	11	2	7
4	5	16	9	4	5	16	9
15	10	3	6	15	10	3	6
1	8	13	12	1	8	13	12
14	11	2	7	14	11	2	7
4	5	16	9	4	5	16	9
15	10	3	6	15	10	3	6

1	8	11	14	1	8	11	14
12	13	2	7	12	13	2	7
6	3	16	9	6	3	16	9
15	10	5	4	15	10	5	4
1	8	11	14	1	8	11	14
12	13	2	7	12	13	2	7
6	3	16	9	6	3	16	9
15	10	5	4	15	10	5	4

FIG. 13

FIG. 14

A few points remain. First, the grouping into α, β and γ sets of 36 major distinct squares cuts right across the classification usually given. Each of these sets of 36 has in it two examples of the type that has been called " Nasik " or " Panmagic ". These are characterized by the fact that, when written as an extended number lattice, one can take a square 4 by 4 *anywhere* and find it is Super Magic. In all other cases of algebraic squares the edges of the squares *must* lie on the lines shown in Fig. 1. The Nasik, or Panmagic, squares can be quickly recognized all pairs adding to 17 being placed as the 1 and 16 in Figs. 7a and 7b, assuming, of course, that one has tested to see it is algebraic. They form an interesting link between the three sets of 36 since, by shifting the edge of the 4×4 square by just *one* row or column (not both) one gets from one set to another. The three Nasik sets can be changed one into another by applications of Fig. 7b (noticing that any ' 16 ' is similarly placed to four ' 1 's).

This property of the Nasik or Panmagic squares has two points of interest. By a shift of just one instead of two, one can get from one of the sets of 36 to another. There are, in fact, only three Nasik or Panmagic number lattices, though each represents 128 magic squares if one includes all mirror images, rotational forms, etc.! These three lattices are given in Fig. 13 since they are needed to show their other amusing property. We can cut at least 10 different " masks " to show four figures through them so that, whichever lattice they are placed on, whichever way up and wherever they are placed, the four numbers showing add to 34. Fig. 14 gives two such masks, selected as being less obvious than mere straight sets or blocks.

To return to algebraic squares in general ; though there are many ways of selecting four numbers from the numbers 1 to 16 so that they add to 34, only nine of these come into one of the twenty-four " standard " sets adding to 34 referred to above. Those containing 1 are :

(*a*) 1, 4, 13, 16	(*b*) 1, 4, 14, 15	(*c*) 1, 6, 11, 16	(*d*) 1, 6, 12, 15
(*e*) 1, 7, 10, 16	(*f*) 1, 7, 12, 14	(*g*) 1, 8, 10, 15	(*h*) 1, 8, 11, 14
(*i*) 1, 8, 12, 13			

and, since three sets that add to 34 cannot have more than one number in common (i.e. where a row, column and diagonal meet at one point), which of these can come in any particular square is severely limited.

It was mentioned earlier that neither 2, 3, 5 nor 9 can ever be in the same row, column, diagonal, etc. as 1. Such limitations can be found quite easily for all numbers. Thus 4, 6, 7 and 16 can never be in the same set as 8. Finally, for anyone who has not played with Algebraic or Super Magic Squares, I personally should recommend these two formulae : The capital letters represent 0, 4, 8 and 12 ; the small ones 1, 2, 3 and 4. + signs are omitted. In the first formula there are no limitations, in the second $A + D = B + C = 12$ and $a + d = b + c = 5$.

Aa	*Bc*	*Cd*	*Db*
Cb	*Dd*	*Ac*	*Ba*
Dc	*Ca*	*Bb*	*Ad*
Bd	*Ab*	*Da*	*Cc*

Aa	*Dc*	*Db*	*Ad*
Bd	*Cb*	*Cc*	*Ba*
Cd	*Bb*	*Bc*	*Ca*
Da	*Ac*	*Ab*	*Dd*

FIG. 15

If one wants them in the form shown in Fig. 2, then one takes always $A=0$ and $a=1$, thus saving much work. The chief drawback to this method is that, though it *does* give all possible algebraic or super magic squares, there appears to be no easy or direct proof that it *has* done so !

Some Super Magic Squares of the Eighth Order

I shall not attempt any exact definition of a Super Magic Square, since the extra properties vary from one order to another. Here is a typical sample of the eighth order :

1	32	27	6	37	60	63	34
55	42	45	52	19	14	9	24
38	59	64	33	2	31	28	5
20	13	10	23	56	41	46	51
25	8	3	30	61	36	39	58
47	50	53	44	11	22	17	16
62	35	40	57	26	7	4	29
12	12	18	15	48	49	54	43

FIG. 16

The rows, columns, diagonals and broken diagonals all total 260. Each " cell " comes to 130—so two adjacent cells, vertically, horizontally or diagonally come to 260. These are all fairly easy to see, but I noticed that there are two other ways of getting 260 which might easily be overlooked :

(i) $(1+42)+(60+19)+(22+61)+(47+8)=260$. These are the " radiating diagonals " of the four cells diagonally placed with respect to one central cell. It can be seen to be perfectly general, e.g.

$$(64+23)+(5+46)+(43+4)+(18+57)=260$$

(ii) $(42+45+59+64)+(4+22+17+7)=260$. Here we have two 2×2 squares which are separated by a 2×2 square, all three squares being placed along a common diagonal. A special case of this would be two cells so placed. Again, it is perfectly general.

(iii) The square can be regarded as a lattice extending in all directions and any 8×8 square taken from it has all the properties of the original, provided the square is taken along cell borders.

I soon discovered that the number of such squares that I had found how to make was so great as to make it out of the question for anyone to do it without a calculating machine or some such help. At a rough estimate, there might be well over 6^5, but how many would turn out to be duplicates I see no way of finding out. Here is an account of the method :

(1) Imagine all the numbers from 1 to 64 to be made of three numbers, one from each of the sets (*a*) 1, 2, 3, 4 (*b*) 0, 4, 8, 12 (*c*) 0, 16, 32, 48.

(2) Now, making use of what I had learned while, literally, playing about with fourth order squares, and assuming $E+F=H+G$ and $e+f=h+g$, I constructed the formula implied in this scheme :

E	*F*	*F*	*E*	*G*	*H*	*H*	*G*
H	*G*	*G*	*H*	*F*	*E*	*E*	*F*
G	*H*	*H*	*G*	*E*	*F*	*F*	*E*
F	*E*	*E*	*F*	*H*	*G*	*G*	*H*
F	*E*	*E*	*F*	*H*	*G*	*G*	*H*
G	*H*	*H*	*G*	*E*	*F*	*F*	*E*
H	*G*	*G*	*H*	*F*	*E*	*E*	*F*
E	*F*	*F*	*E*	*G*	*H*	*H*	*G*

e	*h*	*g*	*f*	*f*	*g*	*h*	*e*
f	*g*	*h*	*e*	*e*	*h*	*g*	*f*
f	*g*	*h*	*e*	*e*	*h*	*g*	*f*
e	*h*	*g*	*f*	*f*	*g*	*h*	*e*
g	*f*	*e*	*h*	*h*	*e*	*f*	*g*
h	*e*	*f*	*g*	*g*	*f*	*e*	*h*
h	*e*	*f*	*g*	*g*	*f*	*e*	*h*
g	*f*	*e*	*h*	*h*	*e*	*f*	*g*

A	*D*	*C*	*B*
C	*B*	*A*	*D*
B	*C*	*D*	*A*
D	*A*	*B*	*C*

FIG. 17

The (*efgh*) section is a mirror image about a diagonal of the (*EFGH*). The (*ABCD*) is the " knight's move " arrangement and is quite independent.

(3) There I stuck until it suddenly dawned on me that, though I had used the equality $E+F=H+G$ to make this formula, it could now be discarded !

(4) Totalling up the broken diagonals, we find that if we take the equalities (*a*) $F+G=E+H$ (*b*) $f+g=e+h$ (*c*) $A+D=B+C$ not only do the broken diagonals come to 260, but so too do the unexpected totals mentioned earlier !

(5) When it comes to actual substitution, I used four schemes similar to the one given here :

AEe *AGh* *AGe* *AEh*	*AGf* *AEg* *AEf* *AGg*
AFg *AHf* *AHg* *AFf*	*AHh* *AFe* *AFh* *AHe*

FIG. 18

(6) The three sets (1, 2, 3, 4) (0, 4, 8, 12) (0, 16, 32, 48) can be substituted for (*A*, *B*, *C*, *D*) (*E*, *F*, *G*, *H*) (*e*, *f*, *g*, *h*) in any way, subject to the limitations found necessary to make the broken diagonals come out correctly.

(7) The scheme given in step (2) can be varied. The quadrants in the *EFGH* diagram can be put together in two other ways that make it no longer necessary for $E+F$ to equal $G+H$. The corresponding *efgh* diagram is made by taking the mirror image of the new *EFGH* diagram about the diagonal. The *ABCD* diagram is left unchanged. New conditions for making the

broken diagonals come out correctly are needed, and are easily found. This now trebles the number of possible squares to be worked out.

(8) More possible formulae can be obtained from the scheme in steps (2) and (7) by a slightly different means. Rows (or columns) of *cells* can be interchanged. In this case the (*efgh*) is treated just as (*EFGH*), i.e. one does not get a new (*efgh*) by " mirror imaging " a changed (*EFGH*). The (*ABCD*) set has to be changed in exactly the same way. This multiplies the possibilities by 36 at least. In each case new conditions for the broken diagonals have to be ascertained. Sometimes there are no limitations for *ABCD*, thus putting the number of substitutions up still more. In these formulae the limitations for *efgh* do not always correspond with those for *EFGH*, for example we can have $E+F=G+H$ with $e+g=h+f$.

Finally, if from these notes anyone can work out how many such super squares there are, which are really independent, I shall be glad to know the answer. I do not call two different 8×8 squares from the same extended lattice *really* independent.

Brief Outline of the Method of making 432 *Super Magic Squares of the Ninth Order*

In the first place, I will admit that I have not made all the 432 squares, and so it might be found that duplicates did turn up, despite my precautions ! By a super magic square of the ninth order, I mean a square of the ninth order, with the numbers 1 to 81 once and once only each, arranged so that one can get 369 out of it at least 54 ways. Here is the first I made as an example :

7	32	74	26	51	66	18	40	55
44	19	60	36	11	76	52	3	68
48	72	13	37	61	5	29	80	24
28	62	23	47	81	15	39	70	4
65	49	9	57	41	25	73	33	17
78	12	43	67	1	35	59	20	54
58	2	53	77	21	45	69	10	34
14	79	30	6	71	46	22	63	38
27	42	64	16	31	56	8	50	75

FIG. 19

Nine rows, nine columns, 18 broken and normal diagonals, nine " cells " and nine if corresponding positions in the cells are combined (e.g. $13+5+24+43+35+54+64+56+75$)—all these 54 sets add up to 369.

Here is the method : place 9 letters *A* in a blank 9×9 marked in cells 3×3 so that it comes once, and only once, in any one cell, in any column, row or diagonal, and never in the same position in two different cells. Since this last condition means that it will be in the centre of one cell, it is convenient to make this the centre cell. I think six, and only six, ways are possible. As they have a very marked " family likeness " I need only give one here (Fig. 20) though all six will be needed to get all possible final super magic squares.

Now imagine this to be but a small piece cut out of a lattice continuing in all directions ; using exactly the same arrangement, start inserting the letters *B* to *I* inclusive from the centres of the other cells to give Fig. 21. It will be found that much work is saved in steps to come if, while working on the five alternatives to Fig. 20 one always starts the different letters from the centres of the corresponding cells, e.g., taking centre letters out of each cell (from my

own notes, one gets the arrangement of Fig. 23 whichever diagram one starts from). Now copy Fig. 21 in mirror image, changing to small letters, to give Fig. 22.

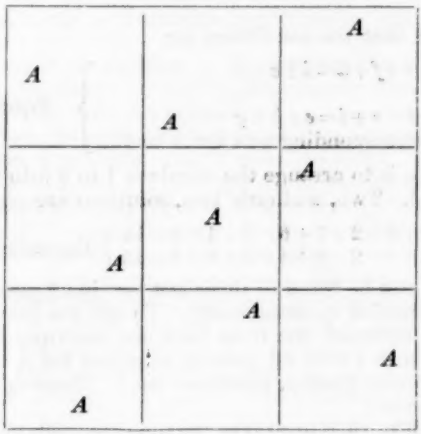

FIG. 20

H	B	G	I	F	C	E	A	D
A	I	D	B	E	G	F	H	C
F	C	E	A	D	H	B	G	I
B	D	I	F	G	E	A	C	H
C	F	H	D	A	I	G	B	E
G	E	A	C	H	B	D	I	F
D	H	F	G	I	A	C	E	B
E	G	B	H	C	F	I	D	A
I	A	C	E	B	D	H	F	G

FIG. 21

d	a	e	c	f	i	g	b	h
c	h	f	g	e	b	d	i	a
i	g	b	h	d	a	e	c	f
h	c	a	e	g	f	i	d	b
e	b	g	i	a	d	h	f	c
f	i	d	b	h	c	a	e	g
b	e	c	a	i	g	f	h	d
a	d	i	f	c	h	b	g	e
g	f	h	d	b	e	c	a	i

FIG. 22

I	E	H
F	A	B
G	C	D

FIG. 23

If a to i are taken as being the numbers 1 to 9, in any order, and A to I as 0, 9, 18, 27 etc. to 72, also in any order, then one has a scheme for making many quite good magic squares—but one still has to find what conditions are to be given the variables so as to make *all* the *broken* diagonals come to 369.

Inspection shows that the conditions are

$$\left.\begin{array}{l} a+h+g=e+f+d=i+c+b \\ \text{and} \\ c+f+h=d+a+i=e+b+g \\ \text{and two corresponding sets for A to I.} \end{array}\right\} \text{Equations I}$$

The next problem is to arrange the numbers 1 to 9 into three triplets, each of which adds to 15. Two, and only two, solutions are possible

$$\left.\begin{array}{l} 1+9+5=2+7+6=3+4+8=15 \\ 1+8+6=2+9+4=3+5+7=15 \end{array}\right\} \text{Equations II}$$

Finding how a to i can be fitted to these last two in all possible ways offers no great difficulty if tackled systematically. To get the corresponding sets for A to I, one would subtract one from each and multiply by 9, then take all possible values for a to i with all possible numbers for A to I and one would get all possible—mirror images, rotations etc. ! There is, I am glad to say, no need to do *all* these !

Turning back to Fig. 19, if one takes the top three cells and puts them on the bottom, or vice versa ; or puts the right-hand three on the left, or vice versa ; or does first one process and then the other, one has still an equally good super magic square. (Only if you crack it along some line other than the cell borders does some of the magic appear to leak out !)

So, any of the squares we want can be rearranged, rotated and reflected so that the cell with the smallest centre is in the middle, that with the next smallest adjacent to its right and the next smallest below it. By adjacent, I mean in this case *not diagonally*.

This can be achieved by conditions (1) $A=0$, (2) B less than C, E or F, (3) $C<E$.

These, coupled with the limitations imposed by equations I and II, luckily for us, leave only one possibility :

A	B	C	D	E	F	G	H	I
0	9	27	36	54	18	45	63	72

But this has to be combined with *all* the possibilities for a to i that conform with equations I and II—and used on all the 6 formulae that one gets, similar to Figs. 21 and 22 but starting from the alternatives to Fig. 20. There are 9 possibilities for a, one can then pick any other (I chose e) in 4 ways (one finds 5 possibilities closed when a is fixed). Then equations I can be fitted to equations II either way. So grand total of super magic squares you should get is $6 \times 1 \times 9 \times 4 \times 2 = 432$. 6 is for the number of formulae, corresponding to alternatives to Fig. 20, 1 is for the fact that one need only take one possibility for A to I. 9 is for the 9 possibilities for a, 4 is for the 4 possibilities for, say, e, and 2 is for the 2 ways of fitting equations I to II.

All of these 432, as far as I can see, are *really* independent. Each can be reshuffled to give 9 that might pass as independent, and each of these can be rotated and reflected to give 8—so with *all* variations one should have 432×72 —I admit that I have not tried to see !

Note 1. The grouping into triplets in equations I are the diagonals and broken diagonals of Fig. 23—hence the importance of the advice given about starting B to I from similar cell centres in the 6 developments from the alternatives to Fig. 19.

Note 2. Some of the alternative formulae to Fig. 21 have strange coincidences which might be worth investigating. My attention was drawn to them by seeing *HAG* written on the top of the left, bottom of the right and again in the dead centre—investigation showed that there were 18 such repetitions, vertically and horizontally, but no other quite so calculated to catch ones eye !

In conclusion, I must thank Miss E. M. Renwick for her helpful and encouraging correspondence and to Dr. G. A. Garreau for his very very great help in getting my notes into order for printing.

[*Note.* Since writing the above, I discovered that some of these ninth order squares are far more wonderful than I ever realised ! If one chooses the variables so that the centre is 41, then one gets a *much* smaller number (96 I think) of *much* more interesting squares of which Fig. 19 is a sample. Here any two numbers placed diagonally opposite and equidistant from the centre add up to 82. In addition *any* 3 by 3 square adds up to 369 and *not* just the ones I made to do so. Even more unexpected, I found that any 3 by 3 square with what might be described as single square spacing, or with double square spacing etc., . . . in fact I think that there are at least 288 ways of getting 369 without deliberately making use of the pairs that add up to 82 : this includes cases such as 3 by 3 squares with three square spacing, of which only one does not, as it were, go off one edge and re-appear on the other side ; the one normal case being that of four corners, four mid-points of sides and centre of square.]

C. DUDLEY LANGFORD.

CORRESPONDENCE

To the Editor of the *Mathematical Gazette*.

DEAR SIR,—Readers will probably be interested to hear that the design in Note 2530 is illustrated on page 101 (Fig. 124) of *New Mathematical Pastimes*, by Major P. A. Macmahon (Camb. Univ. Press, 1921). The author goes on to show how the design can be made the basis of many other pavement types of pattern. The equal-sided pentagon that will fit in this way can, I think, be most easily drawn thus. Construct a right-angled isosceles triangle CRQ, right-angled at C. On RQ construct a triangle RQP having $QP = QR$ and $RP = CR = CQ$. On QP draw a right-angled isosceles triangle QPA having a right angle at A. Then of necessity $QA = AP = RP = CR = CQ$. This uses the figure on p. 104 (Fig. 127) of Major MacMahon's book, but not his method. It seems to me surprising that no firm has ever made coloured glazed tiles of this shape since they are so " versatile " : maybe they would need more than the average labourer to lay them successfully!

Yours etc., C. DUDLEY LANGFORD.

THE HISTORICAL ASSOCIATION, 1906–1956

The jubilee of the Historical Association has been marked by the publication of a pamphlet (56 pp., 2s. 6d., George Philip & Son, Ltd.) describing the origin and development of that Association. Its organisation, with meetings in and out of London, vigorous local Branches, journal and reports, suggests a parallel with our Association ; its membership, some 8000, an example to be imitated.

THE n PRISONERS

By T. J. Fletcher

Most mathematicians have encountered the Problem of the Three Prisoners. Kraitchik [1] gives the problem and the solution as follows :

" Three unsuccessful revolutionists managed to escape across the border, where they were interned in the lockup of a small town. The sheriff's sympathies lay with the prisoners, and he sought a way to let them go free. One day he came into the jail brandishing three white discs and two black ones and said to the prisoners : ' On the back of each of you I shall pin one of these discs. None of you can see his own disc, though you can see your companions ! If any of you guesses correctly that his own disc is white he will receive his freedom. If not, he will be detained indefinitely.' With this he pinned a white disc on the back of each and left them in the care of a guard. How can each of the prisoners, A, B, and C, guess whether his disc is white?

A reasoned : ' The discs of my companions are white, so my disc may be either white or black. Suppose it were black. Then B would have gone to claim his freedom. For he would have said to himself : " Since A's disc is black and C's is white, mine can be either. But it cannot be black, since then C would have seen that our discs were black and would have known that his must be white, since there were only two black discs. But since C has not gone to claim his freedom he must think it possible that his is black, that is, he must see that mine is white." But B has not gone to claim his freedom, so mine must be white.' All three prisoners arrived at this conclusion simultaneously, and all three escaped.

If A had completed his reasoning before the others, they would not have been able to escape. For A's escape would be possible if just one of their discs was black, so each would be tormented with the fear that his was black."

The problem would be better stated if the prisoners were asked simply to deduce the colour of their discs, and not to guess correctly if their discs were white. There are other variants of this problem, but they are too similar to the above to require separate treatment. This paper considers certain assumptions which are implicit in the solution given, and shows that if these be granted more general problems of this type can be solved by similar reasoning.

It is first necessary to consider the original problem in some detail in order to bring out the features which are characteristic of the more general problems considered later. We must consider not only the problem actually presented to the prisoners, that is the problem when the three white discs are used, but also the other problems which might have been set with the same discs. These are simpler, and their solution is needed for the solution of the case which arises in the actual question. A man who can see a black discs and b white discs will be said to be in *state* (a, b). The state (2, 0) will be called an *optimum state*, or a *white optimum*, because anyone in this state can decide the colour of his own disc (white) at once. The other states will be termed more or less *simple* according to the extent to which they depart from the optimum state. Thus the state (1, 1) is one state removed from the optimum. Anyone in this state may employ the argument " If I am black, then the man with the white disc is in the optimum state." The other possible state is (0, 2), and this is the least simple of the three. All the prisoners actually were in this state, and had to employ the argument, " If I am black then the others are in state (1, 1) which is a simpler state than my own, and if they were in that state they could have decided already what colour they were." Several features should be observed. In the first place if the three prisoners are not all in the same state then any prisoner in a less simple state has no opportunity whatever to deduce

the colour of his disc by logical argument. If he gets the colour of his disc it can only be by guesswork. If two blacks and a white are given out, then the only man who can reach a legitimate conclusion is the man with the white disc. If one black and two white are given out then the men with the white discs stand an equal chance, and the man with the black disc stands no chance at all.

The original problem assumed that all the men were *equally intelligent*, whereas one man will win if he is *slightly more intelligent*. These phrases seem to beg a great many questions. (What happens for example if the prisoners have the present theory explained to them before the test begins?) If the prisoners are not all equally intelligent then it is quite possible for a man to be *too clever*. Thus if A and B have black discs and C has a white disc A might say, " If I was black, then C would see two black discs, and so would know at once that he is white. He has not recognised this, so I may conclude that I am white." It might happen that C was a very slow thinker, and had not recognised that he was white for this reason. A would then reach a false conclusion because he was too clever. It seems that in practice, with sophisticated prisoners who have studied the theory, a stalemate might well arise because everyone was afraid of being too clever. The situation can be made more precise by introducing the idea of *decision times* : that is, the time which a given prisoner takes to come to a correct decision when confronted with a given situation. A prisoner must decide his state, allow the decision time for the other prisoners for simpler states to elapse, and then speak up. In practice one is very unlikely to have the relevant information about decision times, and this introduces an air of artificiality into the problem. It is however, interesting to assume that the prisoners have this information, or to assume what comes to much the same thing, that no one will be too clever, and see what decisions about the colour of discs can be reached in more complicated cases. In other words we will assume that the solution given to the original problem is legitimate, and see what further problems we can solve by similar methods.

We will consider the problem of n prisoners with less than n black discs, and n, or more, white discs. There is no point in having more than n white discs, and if there are less than $n-1$ black discs this only makes the situation easier. Therefore the standard two colour problem with n prisoners may be taken to be the problem with $n-1$ black discs and n white ones. $(n-1, 0)$ is an optimum state ; the only people who can reach a legitimate decision are those whose state is nearest the optimum, and they will be white. They must allow a decision time appropriate to a state one simpler than theirs to elapse, and they may then announce that they are white. The game will always be won by a man with a white disc, anyone with a black disc does not stand a chance. The reasoning for this follows the same lines as for the game with three prisoners.

It is interesting to consider what happens when there are less than n white discs as well as less than n black. Let us take the case when there are $n-1$ of each. The game is now two-sided. There two optimum states, and now the blacks do stand a chance. The decision procedure which a prisoner must adopt is as follows. He must count the black and white discs which he can see and decide his state. He knows that all the other prisoners of one colour are in the same state as he is, and that all the prisoners of the other colour are in a neighbouring state. He must decide which is the nearest optimum, and allow sufficient time for anyone in a state one nearer to this optimum to reach a decision, if no one does so he may then announce his colour, which is white if the nearest optimum is $(n-1, 0)$ and black if it is $(0, n-1)$.

The two-sided problem involves fresh subtleties about decision times. Consider, for example, the problem of six prisoners with five white discs, and five black. Suppose that white discs are attached to A, B, and C ; and black

discs to D, E and F. Following the solution of the original problem A is supposed to say, " I am in state (3, 2) ; if I were black B (and C) would be in state (4, 1) and he would argue—' I am in state (4, 1), and if I were black C would be in state (5, 0), and would know immediately that he was white. He has not recognised this, so I must be white.' But B has not done this so I must be white." D, E and F may conclude that they are black in the same way ; and if all six are equally intelligent they will arrive at their conclusions simultaneously. Observe, however, that the inference which each man makes involves the decisions only of those prisoners bearing discs of the same colour as his own. This result is general ; and it may be expressed by saying that all legitimate decisions are *monochromatic*. It introduces new complications if the prisoners are not all equally intelligent. If A, B and C were quicker thinkers than D, E and F they could reach a legitimate conclusion before D, E and F. But this is not all. Let us consider the prisoners and discs as before ; that is six prisoners, five white discs and five black. This time however let white discs be attached to B and C, and black discs to A, D, E and F. A is in exactly the same state as before ; and, of course, now D, E and F are in the same state as he is, while B and C are in the simpler state (4, 1). Being in exactly the same state as before A could start as before by assuming that he was black, and seeing that if he was B and C could decide that they were white. He may know however that B and C are very slow thinkers ; he must therefore ask himself what D, E and F are thinking. In this connection A will say, " I am in state (3, 2). If I am white then D, E and F are in state (2, 3), while B and C are, like me, in state (3, 2). The argument above then shows that D, E and F can decide their colours before myself and B and C, because they are a quicker thinking team. If they have not come to this decision after a suitable interval of time I may conclude that I am black."

In other words if A is confronted with the situation (3, 2) the decision he must make depends on the reaction times of the people bearing the white and black discs which he can see. If the people with white discs are normal thinkers he will conclude that he is white, whereas if the blacks are sufficiently quick thinkers to come to a decision about (3, 2) before the whites can come to a decision about (4, 1) then he will conclude that he is black. It should perhaps be emphasised, before continuing, that in describing how a prisoner argues from his state to the colour of his own disc we are describing his thoughts on the occasions when he is successful. If he is in a situation where he is bound to lose his train of thought is interrupted before it is finished by one, or more, or perhaps all of those with discs of the opposite colour speaking up correctly.

We will now consider what happens when discs of three colours are employed. The state of each prisoner can be specified by three numbers (the numbers of discs of the three colours which he can see) whose total is $n-1$. The states can therefore be represented conveniently on a diagram using trilinear coordinates. We will take the colours of the discs to be red, black and white. When the number of discs of any colour is less than $n-1$ a boundary line is drawn on the diagram parallel to the corresponding side of the triangle of reference, and the states which can actually occur are represented by the points in and on the area bounded by the sides of the triangle of reference and the boundary lines. Figure 1 shows the diagram for eight prisoners with three red, four black and eight white discs ; and figure 2 shows the diagram for ten prisoners with three red, five black, and six white discs. Optimum states, if any, occur where a pair of boundary lines meet inside, or on, the triangle of reference ; because a player in such a state can see all the discs of the two colours concerned, and so can conclude that his disc is of the remaining colour. The distance of a state from an optimum can be measured by the number of steps along lines of the lattice necessary to reach it, and contour lines may be drawn through states of equal simplicity. Algebraically the dis-

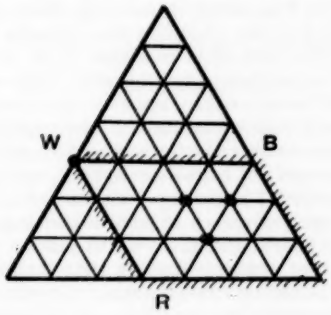

FIG. 1.

tance from, say, a white optimum is equal to the difference between the number of white discs actually seen, and the number which would be seen at the optimum. When discs of three colours are given out to the prisoners the points representing their three states are the vertices of a triangle of unit side, antihomothetic to the triangle of reference. When discs of only two colours are given out we have a pair of points on one of the sides of the triangle of reference ; and when discs of only one colour are used the one state occurring is at a vertex of the triangle of reference.

Consider the problems to which figure 1 applies, and let the discs actually used be two red, two black, and four white. The prisoners with red, black and white discs are, respectively, in states (1, 2, 4), (2, 1, 4) and (2, 2, 3) ; and of these the whites are nearest to the only optimum which is the white optimum at (3, 4, 0). The whites therefore are the only people who stand a chance. The argument may be expressed as a two colour argument, and for the moment the discs may be thought of as " white " and " not-white ". Looking at it this way we see that the previous arguments apply, that the whites may

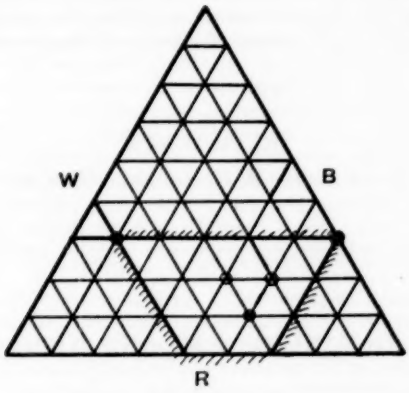

FIG. 2.

determine their colour, and that their decision is monochromatic. Inspection of the diagram shows what is otherwise obvious, that the most difficult problem that can be set with this set of discs is to give white discs to all eight prisoners.

The situation in figure 2 is more interesting since there are two optima, situated at (3, 5, 1) and (3, 0, 6). Let the prisoners be labelled two red, three black and five white. They are then in states (1, 3, 5), (2, 2, 5) and (2, 3, 4) respectively ; and the blacks should win because they are nearest to the black optimum at (3, 0, 6). If however they are slow thinkers, and the whites know this, then the whites may win by using the optimum at (3, 5, 1). The reds have no chance whatever as there is no red optimum.

As a final example we will put ourselves in the position of a prisoner. That is to say we will assume that I know my own state but not anyone elses. We will use the same discs and diagram as in the last example and assume that I can see one red, four black and four white discs ; that is, I am in state (1, 4, 4). I must start my analysis hoping that I can finish and arrive at a correct conclusion. If, however, the situation is against me someone else (all the people of another colour if they are all equally intelligent) will answer first, and I shall have lost the game. I see from the diagram that I am three states from the white optimum, and four states from the black. There is no red optimum, so if I am red I do not stand a chance. Assuming equal intelligence my course of action is to allow time for those only two states from the white optimum to come to a decision, and if they do not come to one to announce that I am white. If however I know that the whites which I can see would take longer to decide this state than the blacks would take to decide a state three away from the black optimum, then I must allow time for this decision to be reached, and if it is not reached I must announce that I am black.

The extension to k colours ($k \leq n$) should now be clear. With a given set of discs an optimum is to be associated with a certain colour if the total number of discs not of that colour is less than n. The distance of a state from an optimum is given by the difference between the number of discs of the associated colour actually seen, and the number which would be seen at the optimum. Having determined his state and the nearest optimum, if all the prisoners are equally intelligent, a prisoner must allow time for anyone a state nearer the optimum than he is to speak up. If no one does so he is to conclude that the colour of his disc is that corresponding to this optimum. When the prisoners are not equally intelligent complications arise again, and the general problems involving the decision times are complex, and might well repay investigation ; but they are beyond the scope of the present paper.

T. J. F.

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GLEANINGS FAR AND NEAR

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" Free hitting was the order of the day . . . ; business men . . . paid out £35 10s. to charity, the Englishmen costing them £20 for three sixes and 31 fours and the Australians £15 10s. for five sixes and 16 fours." Reuter report on the match between the M.C.C. and the Prime Minister's XI at Canberra, 8 Dec. 1954. [Per Mr. A. P. Rolet.]

MATHEMATICS AND THE MIND

By G. Patrick Meredith

Our common use of the word " mind " invests the term with considerable obscurity and ambiguity. This is partly because, whilst many philosophers, psychologists and scientists deny any reality to the notion, many others have regarded it as the only reality. Others again have regarded both mind and matter as having reality but reality of quite different types, e.g. subjective reality and objective reality. Without going into these controversies I must begin by pointing out that for a psychologist, whatever his philosophical beliefs, the mental processes of the people he is studying are as real and as objective as the interior of a star is to an astrophysicist—and as inaccessible to direct observation. Any other assumption would make nonsense not only of psychology but of the whole of education. And it has a special bearing on the problem of determining the nature of mathematics.

Knowledge is something which happens to man, a mental achievement which he works for, carries about with him and uses. It is therefore of special psychological interest. Some kinds of knowledge at first sight seem to present more problems than others. On the whole the various types of practical knowledge seem relatively easy to understand psychologically. Observational knowledge, our sensory acquaintance with objects or events, also is not too difficult. Verbal knowledge looks like a more subtle kind of practical knowledge, but there are hidden difficulties here, for except with parrots and budgerigars, speech is not merely the ability to make articulate sounds. It demands the operation of syntactical rules. And this shades imperceptibly into ideational or theoretical knowledge, which includes mathematics.

The special problem of mathematics is this : practical knowledge means the ability to do something definable ; observational knowledge is the ability to recognise or recall the appearance or sensory properties of something definable ; verbal knowledge means the ability to recall, recognise or articulate sounds having ascertainable meanings ; but what is mathematical knowledge *about*? We can ignore all its practical *uses*—these are applications but the very fact that one and the same equation may be applied to a wide variety of physical occurrences shows that its meaning cannot be specified by any of them. The symbols of mathematics do not signify material objects or physical processes. The temptation is to regard them as referring to something transcendental. Thus Hardy in *A Mathematician's Apology* declares : " I believe that mathematical reality lies outside us, that our function is to discover or *observe* it, and that the theorems which we prove, and which we describe grandiloquently as our ' creations ' are simply our notes of our observations."

He thus took his stand on the Platonic view. The reality with which mathematics is concerned is, on this view, neither physical nor psychological but transcendental.

The case for this view is very strong. The reality of mathematics cannot be physical because (1) mathematical theorems are not discovered by processes of physical observation or manipulation, and (2) the great bulk of pure mathematics has no known physical applications. The reality of mathematics cannot be psychological because (1) the creative mathematician works within a framework of logical constraints which are public and independent of his personality and (2) although mathematical discoveries are necessarily made by psychological processes no resemblance between these processes and the content of the theorems is discernible.

The transcendental view is one which I am almost tempted to hold myself. If there is a physical reality and a separate mental reality then

mathematics cannot be located in either, and requires a third realm of its own. But we should remember William of Ockham and ask ourselves whether all these realms of existence are strictly necessary. Contrariwise we might also ask whether Ockham's razor was strictly necessary. There is no guarantee that the Universe is ultimately simple. And if we need as many as three distinct realms of existence, why not six or twenty or a thousand? But do we need any at all?

In *What is Mathematics?* R. Courant and H. Robbins say

"Whatever our philosophical standpoint may be, for all purposes of scientific observation an object exhausts itself in the totality of possible relations to the perceiving subject or instrument. Of course, mere perception does not constitute knowledge and insight; it must be co-ordinated and interpreted by reference to some underlying entity, a "thing in itself," which is not an object of direct physical observation, but belongs to metaphysics. Yet for scientific procedure it is important to discard elements of metaphysical character and to consider observable facts always as the ultimate source of notions and constructions. To renounce the goal of comprehending the "thing in itself," of knowing the "ultimate truth" of unraveling the innermost essence of the world, may be a psychological hardship for naive enthusiasts, but in fact it was one of the most fruitful turns in modern thinking."

I would agree with Hardy that mathematics is not about material objects, nor about mental states. I also think it is something very special and very powerful, or rather that it is especially *general* and consequently especially powerful. The power of its applications is merely a consequence of its intrinsic power. The fact of its applicability does, however, indicate that its nature cannot be altogether independent of physical reality. The fact of its origins in the mind of the mathematician likewise indicates that it cannot be altogether independent of psychological reality. But the fact of the non-application of the greater part of it together with the fact that there is no knowing when any hitherto unapplied theorem may find an application must also be reckoned with.

These four facts seem to me highly important and significant for any discussion. To me they suggest that mathematics is somehow concerned with both physical and psychological reality not only as they are but as they might be. Mathematics can be described, though not defined, as the exploration of possibilities by processes of logic. This agrees well with the definition in *Essays in Science and Philosophy*, by A. N. Whitehead:

"*Definition of Mathematics*—It has now become apparent that the traditional field of mathematics in the province of discrete and continuous number can only be separated from the general abstract theory of classes and relations by a wavering and indeterminate line. Of course a discussion as to the mere application of a word easily degenerates into the most fruitless logomachy. It is open to any one to use any word in any sense. But on the assumption that 'mathematics' is to denote a science well marked out by its subject matter and its methods from other topics of thought, and that at least it is to include all topics habitually assigned to it, there is now no option but to employ 'mathematics' in the general sense of the 'science concerned with the logical deduction of consequences from the general premisses of all reasoning.'

But it may be objected that the nature of mathematics is the concern of the mathematician, not of the psychologist. To this there are three answers. In the first place the psychologist is increasingly making use of mathematics and he ought to take an interest in the nature of his instruments. Secondly mathematics, whatever its nature, is a production of men's minds, or, if you like, a form of human behaviour and, as such, it is as much the concern of the

psychologist as education, crime, love-making or neurosis. Thirdly the psychologist has methods of tracing the growth of mathematical ability in children and this may throw some light on the nature of mathematics itself.

Mathematics is commonly regarded as an abstract subject. If Hardy's view is correct mathematics cannot be abstract. For abstract knowledge means knowledge arrived at by abstracting from observed phenomena. Hardy declares that mathematics is itself observational knowledge, albeit of a non-physical reality. His use of the term " observation " is evidently not intended to be metaphorical or at any rate only partly so. He would doubtless say that although we do not observe mathematical reality through any of our sense-organs we can still be said to observe it through our intellect. Mathematics is not then a series of intellectual constructions but a series of intellectually discovered facts.

There is this much to be said for Hardy's view namely that as perceptual facts have a character of external compulsion about them i.e. they must be accepted, they seem independent of our volition, so too have mathematical theorems. Indeed the whole quest for rigour is an attempt to make them completely secure. Nevertheless I, for one, cannot regard the compulsiveness of mathematics as evidence for a transcendental and presumably static type of reality. Its dynamic, progressive character is indicated in *The Nature of Mathematics*, by M. Black, thus:

" The progress of mathematics is not smooth, nor is the science as the layman imagines, a collection of subtle principles and infallible results, springing mysteriously yet convincingly into the minds of their inventors. Its discoveries have, in general, not won immediate or universal acceptance, for mathematics, like every other system of organized knowledge, owes its development to the insight of thinkers whose creative imagination has led them to results which often startled themselves and their contemporaries."

One of the reasons why mathematicians deny any sensory basis to mathematics is that they accept the traditional and popular list of five senses as a correct account of our perceptual equipment. And since mathematics is possible for the blind and the deaf, and certainly has little to do with taste and smell (though touch may not be so easily eliminated) and since mental imagery is very variable and plays little or no part in the mental processes of many mathematicians, it would seem that mathematics is independent of perception. Against this, of course, can be set the undeniable fact that mathematics makes use of symbolism which must be written and read, both involving perceptual processes. Indeed some mathematical philosophers have seized on this very point and assigned to symbols as such the very role we are exploring. As M. Black, in the same book, remarks:

" The view of the nature of pure mathematics inspired by the *Tractatus* may perhaps be not altogether inadequately expressed by saying that pure mathematics is the syntax of all possible systems of symbols."

But there is surely something very paradoxical about declaring that symbols are themselves the *meaning* of mathematics. A symbol is something which *has* a meaning. How then can it *be* a meaning, except in a trivial sense?

The reason why we should think twice before excluding any perceptual content from mathematics is that we have more than five senses. The number is nearer to twenty. And of these there is one which we very commonly overlook viz. the one known as " proprioceptive " which means the sensations of movement and posture. We are liberally supplied in our muscles, tendons, joints and semi-circular canals, with sensory organs continuously reporting to the brain on all the movements we make and all the positions we adopt in space. Without this information no skills would be possible. We could not walk or manipulate or write or speak. Proprioception might therefore be called our *operational sense*.

This leads me to offer an explanation of the apparent non-materiality of mathematics, an explanation which avoids the postulation of a transcendental realm and which squares with the psychological account of the processes by which mathematical knowledge develops in the individual. Most sciences have some form of matter as their basic content. Physics and chemistry have atoms and molecules. Biology has living organisms. Geology has the earth's crust. Astronomy has planets, stars and nebulae. The content of mathematics is not material in this sense. But it is not independent of material bodies. My suggestion is that *operations* constitute the true content of mathematics.

Before pursuing this suggestion I should point out that the very search for *a* meaning to mathematics presupposes that there is a single answer. We have no certain guarantee that mathematics is a unitary body of knowledge. Perhaps we should speak rather of " the group of mathematical studies " a group based on superficial resemblances rather than on fundamental identity of origin. E. T. Bell mentions this very possibility in the following quotation from *The Development of Mathematics*.

" It has been suggested that the ultimate course of mathematics will resemble that followed by geometry after the invention of non-Euclidean geometries. There may be no necessity to have but one kind of mathematics, if the objective is scientific applications ; and there are no reasons for supposing that a single, all-inclusive mathematics can embrace all the useful kinds without internal contradictions. Again, in the same direction, but going deeper, it has been very hesitantly suggested that complete self-consistency is more of a luxury than a necessity in a usable mathematics. Looking far ahead, some whose work entitles them to an opinion believe that the paradoxes which have infested mathematics since Zeno are unsolvable, and that the mathematics of continuity will be abandoned, to be succeeded by a type of reasoning in which unsolvable paradoxes cannot arise. What this is to be, provided it is possible, of course nobody can predict. It may be worth mentioning that one of the most distinguished mathematicians of the twentieth century practically abandoned analysis, in which he had done outstanding work, because he had convinced himself that the foundations of analysis are unsound beyond repair. (This is not the sort of opinion a man cares to publish today ; he might change his mind tomorrow). A last possibility is suggested by a similar situation in quantum mechanics. With the apparent necessity for some form of indeterminacy in the physical sciences, the classical dichotomy ' true, false ' is modified by an admixture of probabilities. In analogy with this, mathematics may cease to insist on a two-valued logic, and develop in accordance with a logic of three or more truth-values."

Perhaps one of the reasons why many mathematicians have resisted the Russell-Whitehead thesis of the identity of mathematics with logic is that it conflicts with their subjective experience. They *use* logic in proving their theorems but the things they think about whilst discovering their theorems do not seem to be identical with the notions of logic but to have an independent nature of their own. The fact that these notions were not analysed in terms of logic until the twentieth century suggests that they had some pre-logical origin. To refer to the logical analysis of mathematics as the " foundations " does seem odd since the foundations have been constructed *after* the building of the main edifice. In point of time they are essentially a superstructure.

In postulating an operational basis for mathematics as distinct from either a logical or a transcendental basis I have to avoid a danger. Human beings are imperfect and their hands are more or less clumsy. Many of their operations lack precision. The same is true of their mental operations. It would lead to absurdities to suggest that mathematics was the science of human work—a science known nowadays as " Ergonomics "—or that it is the science

of mental operations, which would make it a branch of psychology. For similar reasons geometers have come to deny that geometry is concerned with physical space, for actual triangles, circles, etc., are always inexact. Whitehead evidently had such considerations in mind in making the following remark :

" In one sense there is no science of applied mathematics. When once the fixed conditions which any hypothetical group of entities are to satisfy have been precisely formulated, the deduction of the further propositions, which also will hold respecting them, can proceed in complete independence of the question as to whether or no any such group of entities can be found in the world of phenomena."

Now if we examine closely what the physicist is doing we see that whilst recognising the imperfections of his instruments and the consequently approximate character of his measurements he finds, or at any rate postulates, complete precision in the physical entities themselves. A carbon atom has exactly four outer orbital electrons and no probable error. Even Heisenberg's Uncertainty Principle does not imply that particles are themselves vague as to their whereabouts, but only that the more accurately we measure their position the less accurately we can ascertain their momentum. Nature is what it is, exactly itself. And the physicist, in reporting on his work, renders two accounts. The first account describes his instruments and measurements and may supply a more or less erratic graph. The second account is an ideal interpretation of his data in terms of mathematical equations. What are these equations? They express relations not between the actual and more or less imperfect operations performed by the physicist in the course of his experiments but relations between *ideal* operations.

Now counting is an operation, a process of matching one group of objects with another group. Addition, subtraction, multiplication and division are all likewise operations, initially physical, later symbolic and mental. Tracing a line, area or volume, and rotating a vector are likewise operations. Between them these operations form the basis of large areas of arithmetic, algebra and geometry. Weyl has shown that the whole of analytic geometry can be built up on operations with vectors. " To-day probably the best approach to analytic geometry is by means of the vector concept. . . . The translations, or parallel displacements, of space are called *vectors*. . . . Translations can be combined, they form a group . . . Thus arises an axiomatic construction of geometry . . . that presupposes the fully formed concept of real number . . . and uses as the only basic geometric concepts ' point ' and ' vector '. "
(Hermann Weyl, *Philosophy of Mathematics and Natural Science*)

Mathematical symbols, then, refer to operations, to combinations, to relations between operations. But of course the crux of our problem lies in the necessity for speaking of " ideal " operations. For this seems to admit the very transcendental realm we have been rejecting. If ideal operations, why not ideal points, lines, figures, numbers and the whole platonic heaven? I think this objection can be answered completely though I shall give only a partial answer here.

In the first place we can approximate more and more closely to a perfect performance of an operation whereas we can never approximate to points, lines, etc. Our pencil drawings may *represent* points, lines, etc. but they can never satisfy the definitions of these entities and they differ fundamentally from them. But an operation is a procedure for bringing about a changed relationship of some kind and this in fact is what our manual operations do achieve. There is no discontinuity between the actual and the ideal operation. In fact it would be better to speak of " ideally defined operations " rather than " ideal operations ". For the important aspect of an operation, especially for mathematics, is the changed relationship brought about by it, not the degree

of clumsiness in the performance. We may shut a door gently or bang it : the result is physically the same.

In the second place the reasons which make mathematicians resist any attempt to assign a *material* basis to mathematics (one of which is that although the manifestations of matter are exceedingly varied the theorems of mathematics are far more so and thus mathematics is too rich to be founded on a material basis), are reasons which do not hold for operations. For the number of possible operations between any group of bodies far exceeds the number of bodies, so that we can envisage the occurrence of as many operations as there are theorems and every theorem can, in principle, be translated into operational terms. But this does not imply that the operations are in all cases physically practicable. In fact the majority of operations constituting the meaning of mathematics are not practically performable but correspond with *mental* operations.

Aha ! *Mental?* Are we not then smuggling in the transcendental ideal? Here we return to my starting-point which was the misleading of the word " mind ". It is precisely this false equation " mental = non-material = ideal " which is the root of all the confusion about the nature of mathematics. Mental processes really occur. There is nothing ideal or transcendental about them. When children sit in an arithmetic class doing sums their minds are not in Plato's heaven. They are combining mental but quite earthly operations and symbolizing the outcome. A few of them will one day become mathematicians. Will their mental processes at some stage suddenly be transported from the terrestrial to the celestial? If so when? After G. C. E., or upon graduation or after election as a Fellow of the Royal Society? Is it not more rational to assume a continuity of fundamental content under the proliferation of patterns?

Nevertheless the apparent contrast between physical and mental operations may still occasion serious doubts as to the validity of this operational theory. Would not this contrast import a duality into the heart of mathematics? Indeed it does. It creates the corresponding contrast between applied and pure mathematics. Applied mathematics refers to possible physical operations in the material world. Pure mathematics refers to mental operations. But this is not an absolute and unbridgeable duality. The bridge, if I had time to elaborate on its construction, would be found in that proprioceptive sense to which I made earlier reference. Briefly, primitive man learned to operate manually before he got far with any mental operations. Through his proprioceptive sense the manual operations associated with his early crafts became internalized. In addition to his stock of visual and other sensory images he thus acquired a stock of operational " images ". These differed from the rest by being essentially dynamic in character. He found himself not only performing actions but envisaging actions. He began, in other words, to *think*. Hence mental operations, whilst differing in many respects from manual operations, were derived from the latter and are still organically related to them. And it seems to me that this view, which is an elaboration of the penetrating declaration of that greatest of physiologists Sir Charles Sherrington that " Mind, recognizable mind, seems to have arisen in connexion with the motor act ", does throw some light on the relation of mathematics to mind.

G. P. M.

FOR SALE

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THE NEDIANS, THE NEDIAN TRIANGLE AND THE ALIQUOT TRIANGLE OF A PLANE TRIANGLE

By John Satterly

This is a continuation of a note in the *Gazette* of May, 1954.

Medians

Let ABC (Fig. 1) be a plane triangle and AD, BE, CF its three medians of lengths m_1, m_2, m_3 respectively. The medians are concurrent at the centroid G.

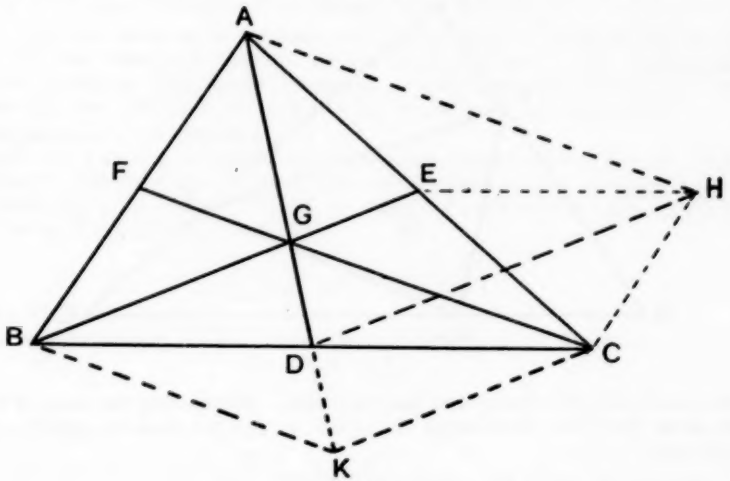


Fig. 1

It is well known that

$$m_1^2 + m_2^2 + m_3^2 = \tfrac{3}{4}(a^2 + b^2 + c^2) \tag{1}$$

but not so well known that

(2) the area of the triangle whose sides are the medians, namely the $\triangle ADH$ formed by drawing DH, AH parallel to BE and CF respectively to meet in H, equals $\frac{3}{4}$ (area of $\triangle ABC$).*

Note the common factor $\frac{3}{4}$ in (1) and (2).

Coordinate geometry may be used to get the area of the $\triangle ADH$. A simple geometrical proof is obtained by first drawing BK, CK parallel to CF, BE respectively to meet AD produced in K. The medians divide $\triangle ABC$ into six equal areas and the $\triangle$s BDK, CDK add two more. The triangle GKC has two of these areas and GK, KC, CG are equal to $\frac{2}{3}m_1$, $\frac{2}{3}m_2$, $\frac{2}{3}m_3$ respectively. The $\triangle$'s ADH, GKC are similar and therefore their areas are as the squares of their homologous sides (*Euc.* VI, 19). Thus

$$\frac{\triangle ADH}{\triangle GKC} = \frac{AD^2}{GK^2} = \left(\frac{3}{2}\right)^2$$

and so

$$\triangle ADH = \tfrac{9}{4}(\triangle GKC) = \tfrac{9}{4}\,\tfrac{1}{3}(\triangle ABC) = \tfrac{3}{4}(\triangle ABC)$$

Note that HE, HC are parallel to BC, AB respectively.

* Given as an exercise in E. W. Hobson : *Plane Trigonometry* (Camb. Univ. Press, 3rd edition, 1911) p. 201, § 159, No. 3.

2. *The Aliquot Triangle or the* 1/*Nth point-division triangle*

This triangle, the $\triangle DEF$ of Fig. 2, is obtained by joining the points D, E, F which divide BC, CA, AB respectively such that

$$\frac{BD}{BC}=\frac{CE}{CA}=\frac{AF}{AB}=\frac{1}{N}.$$

(In Fig. 2 the value of $N=3$).

Denote the lengths of its sides by d, e, f. In each of the $\triangle$'s BDF, CED, AFE we know two sides and the included angle. Hence we can get the square

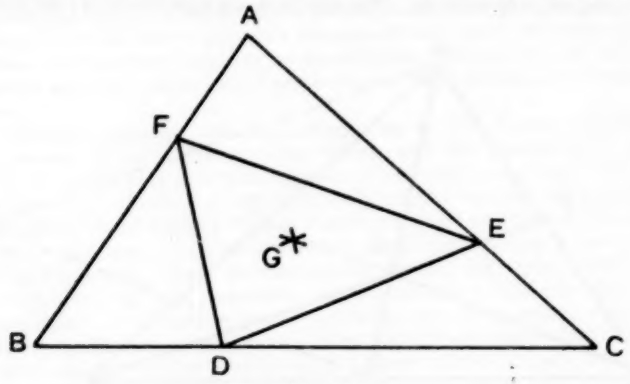


FIG. 2

of the third side and the area of each triangle. Subtracting the sum of these three areas from the whole area of $\triangle ABC$ we get the area of $\triangle DEF$. The results are :

(1) $d^2+e^2+f^2=(N^2-3N+3)(a^2+b^2+c^2)/N^2$

(2) area of $DEF=(N^2-3N+3)$(area of $\triangle ABC)/N^2$

(Note the common factor in (1) and (2)) : and

(3)† also the centroid of $\triangle DEF$ is common with the centroid G of the $\triangle ABC$.

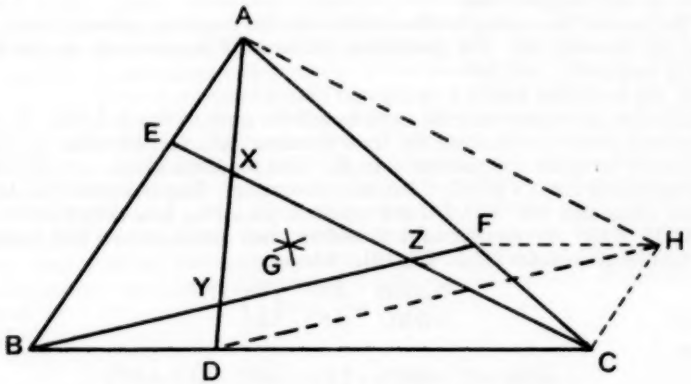


FIG. 3

† Discovered by Pappus : see W. W. Rouse Ball, *A Short History of Mathematics* (Macmillan, 6th edition., 1915) p. 100.

3. *The Nedians*

These are the straight lines AD, BE, CF (Fig. 3) joining A, B, C to the points D, E, F obtained as defined above. Denote the lengths of AD, BE, CF by n_1, n_2, n_3 respectively. The squares of AD, BE, CF may be easily obtained from the $\triangle$'s ABD, BCE, CAF and we find

$$(1) \qquad n_1^2 + n_2^2 + n_3^2 = (N^2 - N + 1)(a^2 + b^2 + c^2)/N^2.$$

(2) The area of the triangle formed by taking the nedians as sides namely the $\triangle ADH$ of Fig. 3, where DH, AH have been drawn parallel to BE and CF respectively,

$$= (N^2 - N + 1)(\text{area of } \triangle ABC)/N^2.$$

There does not seem to be an easy geometrical proof of this similar to that above for the triangle formed by the medians as sides but the expression is obtained easily by Coordinate Geometry. Note again the common factor in (1) and (2), also that HE, HC are parallel to BC and AB respectively.

4. *Intersections of the Nedians*

Let X, Y, Z be the intersections of the nedians two by two (Fig. 3). It can be shown* that the three segments into which each nedian is divided are in the same ratio, i.e. $AX : XY : YD$ as $BY : YZ : ZE$ as $CZ : ZX : XY$, this ratio being $N : N(N-2) : 1$, so that

$$\frac{XY}{AD} = \frac{YZ}{BE} = \frac{ZX}{CF} = \frac{N(N-2)}{N^2 - N + 1}.$$

5. *The Nedian Triangle*

This is the triangle XYZ. Denote its sides by x, y, z. Since

$$\frac{z}{n_1} = \frac{XY}{AD} = \frac{N(N-2)}{N^2 - N + 1}$$

we get at once

$$(1) \quad x^2 + y^2 + z^2 = \left(\frac{N(N-2)}{N^2 - N + 1}\right)^2 (n_1^2 + n_2^2 + n_3^2)^2$$

$$= \left(\frac{N(N-2)}{N^2 - N + 1}\right)^2 \left(\frac{N^2 - N + 1}{N^2}\right) (a^2 + b^2 + c^2)$$

$$= \frac{(N-2)^2}{N^2 - N + 1} (a^2 + b^2 + c^2).$$

Also since the nedian triangle XYZ is similar to the triangle ADH of Fig. 3 formed by taking the nedians as sides and areas of similar triangles are as the squares of their corresponding sides (*Euc.* VI, 19) we get

$$(2) \quad \text{Area of } \triangle XYZ = \left(\frac{N(N-2)}{N^2 - N + 1}\right)^2 (\text{area of } \triangle ADH)$$

$$= \left(\frac{N(N-2)}{N^2 - N + 1}\right)^2 \left(\frac{N^2 - N + 1}{N^2}\right) (\text{area of } \triangle ABC)$$

$$= \frac{(N-2)^2}{N^2 - N + 1} (\text{area of } \triangle ABC)$$

Note again the common factor in (1) and (2).

(3) It may also be shown that the areas of the triangles AFX, BDY, CEZ are equal and equal to

$$\frac{1}{N(N^2 - N + 1)} (\text{area of } \triangle ABC);$$

* Adapted and extended from N. Alliston, *Mathematical Snack Bar*, (Heffers, Cambridge, 1936) Ch. 1, pp 12–13.

also that the areas of the quadrilaterals $FXYB$, $DYZC$, $EZXA$ are equal and equal to

$$\frac{N^2-N-1}{N(N^2-N+1)} \text{ (area of } \triangle ABC).$$

(4) Also the centroid of the Nedian triangle XYZ is common with that of the $\triangle ABC$ and that of the Aliquot $\triangle DEF$, i.e. G, is common to the three triangles.

6. *Relation between the nedian triangle XYZ and the aliquot triangle DEF.*

Denote the intersections of XY and EF, YZ and FD, ZX and DE by P, Q, R respectively (Fig. 4). Figures 1, 2, 3, 4 are all drawn for a N equal to 3.

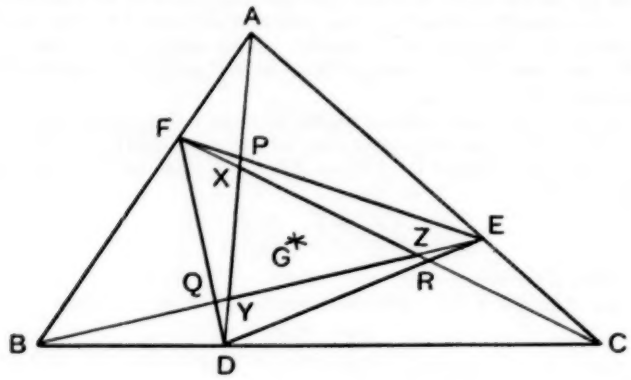


FIG. 4

Careful observation of Fig. 4 showed that

$$\frac{FP}{FE}=\frac{DQ}{DF}=\frac{ER}{ED}=\frac{1}{5}$$

so that apparently the $\triangle XYZ$ which is a nedian triangle of $\triangle ABC$ for a N equal to 3 is a nedian triangle of $\triangle DEF$ for a new value of N, say M, equal to 5. This was verified with triangles of different shapes but of same N. Also careful drawings made of other triangles with N equal to 4 gave values of M equal to 10 and so on.

To settle this relationship let us resort to algebra. We have

$$\frac{\triangle XYZ}{\triangle ABC}=\frac{(N-2)^2}{N^2-N+1} \text{ by (5) 2}$$

$$\frac{\triangle DEF}{\triangle ABC}=\frac{N^2-3N+3}{N^2} \text{ by (2) 2}$$

hence
$$\frac{\triangle XYZ}{\triangle DEF}=\frac{(N-2)^2}{(N^2-N+1)}\cdot\frac{N^2}{(N^2-3N+3)}=\frac{(N^2-2N+2-2)^2}{\{N^2-(N-1)\}\{N^2-3(N-1)\}}$$

$$=\frac{\{(N^2-2(N-1))-2\}^2}{\{N^2-2(N-1)\}^2-\{N^2-2(N-1)\}+1}=\frac{(M-2)^2}{M^2-M+1}$$

which is of the same form as (5) 2, M taking the place of N, where

$$M=N^2-2(N-1) \text{ or } M-1=(N-1)^2.$$

Thus when $N=(2), 3, 4, 5, 6\ldots$, $M=(2), 5, 10, 17, 26\ldots$; a linear relation of N gives a parabolic relation for M. This is possibly a new relationship.

As mentioned in the *Gazette* of May, 1954 we may consider that anti-clockwise motion around a triangle gives " forward " nedians and clockwise motion " backward " nedians. All the results given above hold for both sets of nedians and their associated triangles. It is of interest to note in the above the prevalence of expressions of the form $N^2-i(N-1)$ where i has the values 1, 2, 3.

Some writers may prefer to divide the sides of the triangle into two portions in the ratio m/n instead of using my $1/N$ fractional ratio. The relationship between the two systems is given by $\dfrac{1}{N}=\dfrac{m}{m+n}$. The above expression involving N may be easily translated into the alternative system. Thus :

$$\text{for } \triangle DEF,\ \frac{d^2+e^2+f^2}{a^2+b^2+c^2}=\frac{m^2-mn+n^2}{(m+n)^2}=\frac{\triangle DEF}{\triangle ABC}$$

$$\text{for } \triangle ADH,\ \frac{n_1^2+n_2^2+n_3^2}{a^2+b^2+c^2}=\frac{m^2+mn+n^2}{(m+n)^2}=\frac{\triangle ADH}{\triangle ABC}$$

$$\text{for } \triangle XYZ,\ \frac{x^2+y^2+z^2}{a^2+b^2+c^2}=\frac{m^2-2mn+n^2}{m^2+mn+n^2}=\frac{\triangle XYZ}{\triangle ABC}$$

$$\text{for } \triangle\text{'s } AFX, BDY, CEZ,\ \frac{\text{Area of either } \triangle}{\triangle ABC}=\frac{m^3}{(m+n)(m^2+mn+n^2)}$$

for quadrilaterals $FXYB, DYZC, EZXA$,

$$\frac{\text{area of either quadrilateral}}{\triangle ABC}=\frac{m(n^2+nm-m^2)}{(m+n)(m^2+mn+n^2)},\quad (n>m).$$

Ratio of segments of nedians, beginning at vertex

$$(m+n)n : m^2-n^2 : n^2\quad ,\quad (m>n).$$

Nedian ratio giving $\triangle XYZ$ in $\triangle ABC=N=\dfrac{m+n}{m}$.

Nedian ratio for $\triangle XYZ$ in $\triangle DEF=M=N^2-2(N-1)$

$$=(N-1)^2+1=(m^2+n^2)/m^2.$$

From the last two lines we see that if the first ratio division divides the sides of the original triangle in the ratio $m : n$ the ratio division in which the nedians divide the sides of the aliquot triangle is $m^2 : n^2$.

J. S.

1858. The Size of Hell.

Franciscus Ribera in Cap. 14, Apocalyps. will have hell a material and local fire in the centre of the earth, 200 Italian miles in diameter. . . . But Lessius, Lib. 13, *de moribus divinis*, cap. 24, will have this local hell far less, one Dutch mile in diameter, all filled with fire and brimstone; because as he there demonstrates, that space cubically multiplied will make a sphere able to hold 800,000 millions of damned bodies (allowing each body six foot square) which will abundantly suffice; cum certum sit, inquit, facta subductione, non futuros centies mille milliones damnandorum.—Burton, *Anatomy of Melancholy*, Everyman Edition, II, p. 42. [Per Prof. H. G. Forder.]

THE ARABIC NUMERALS, NUMBERS AND THE DEFINITION OF COUNTING

By R. L. Goodstein

Before men learnt to *count*, the shepherd guarded against the loss of a sheep from his flock by making a *tally* of the flock. As each sheep passed through the gate in the morning, on the way to the grazing land, the shepherd cut a notch in his stick. At nightfall, when the flock returned to the fold, he checked his sheep against the tally by running his finger along the notches, moving from notch to notch as each sheep passed. The tally stick served its purpose just as well as counting the flock serves the shepherd today. The tally could be passed from hand to hand almost as easily as numbers are now passed by word of mouth (or written down); it constituted a reasonably permanent flock record, and so long as flocks remained quite small there was no necessity to mother the invention of a new system of recording.

A very simple modification of the primitive tally stick extended its scope to a remarkable degree. Two sticks, in place of one, served not just to double the size of the flock of which a tally could conveniently be made, but to increase it perhaps twenty-fold. One of the sticks, serving as a standard, carries, say, twenty notches, and the second, separated into two parts by some distinguishing mark, is initially uncut. This modified tally-stick is used in the following way. As each sheep of the flock passes through the gate the shepherd moves his finger up one notch on the standard stick, and when he comes to the top of the standard, he cuts a notch on the lower part of the second stick, and then starts again at the first notch on the standard. Each time his finger reaches the last notch on the standard he cuts a notch in the second stick. When the tally is complete the standard is laid alongside the upper, uncut, part of the second stick and the notches on the standard, up to the last covered in making the tally, are matched against fresh notches cut in the second stick. The second stick now serves just as well as the primitive tally as a guard against loss from the flock, for the process of construction is reversible with the aid of the standard stick; on the return of the flock the shepherd moves his finger from notch to notch on the lower half of the double stick each time he completes a tally on the standard stick, and when the last notch on the lower part of the stick is reached, the standard is discarded and the remaining sheep are matched against the notches in the upper part.

This tally-of-a-tally, as we may describe the two-stick tally, effects an immense economy both in effort and material. For instance the tally of a flock of four hundred and seven sheep will consist of only twenty-seven notches. The number of notches on the standard is, of course, quite arbitrary, and a standard of twenty has no special advantage; the ten fingers on our hands, however, provide a standard so readily accessible and convenient that a standard of ten was adopted almost universally.

The *abacus*, which is a familiar toy in the nursery to this day, is a development of the two-stick tally. The prototype of the abacus consisted of a few vertical wires open at the top to allow the free passage of beads off and on the wires. To simplify the description of operations with an abacus we shall suppose the abacus to be so set up before us that we can talk of a right-hand and left-hand end wire, and we shall think of the wires as ordered from right to left, so that by the term " the next wire " we mean always the next to the left, and by " the previous wire " we mean the next wire to the right.

In making a tally on the abacus the beads take the place of the notches, and the wires that of the sticks. A two-wire abacus is used in very much

the same way as the two-stick tally, the addition of further wires serving to accentuate the economy in expression gained in replacing the primitive tally by the two-stick tally. For the moment we shall suppose that each wire is just long enough to carry ten beads. When the first wire is full a bead is placed on the second wire and the first is then emptied; the next bead is again placed on the first wire, and so on. When the second wire is full a bead is placed on the third wire, and the second is emptied, and the next bead is again placed on the first wire, and when the third is full, a bead is placed on the fourth wire and the third is emptied, and so on. Each wire in turn serves as a standard, the beads on a wire recording the number of times the previous wire has been filled. In reversing the process, we start at the right-hand end, and when the first wire has been emptied a bead is removed from the second wire and the first wire is filled again. When the second has been emptied a bead is removed from the third and the second wire is then filled again, and so on.

After the invention of the abacus the next stage in the development of Arithmetic was the discovery of a method of recording (on sand or paper) a tally made on an abacus. We are so familiar with the process of counting that it is exceedingly difficult for us to realise how deep a problem this recording presented before the invention of counting. The abacus itself takes us a long way towards a solution, for by distributing the beads amongst several wires, a *positional notation* is strongly suggested. In fact the problem is reduced to that of finding a method of recording the (relatively few) beads on a single wire, the complete tally being then expressed by the juxtaposition of the symbols standing for the beads on the single wires. A mere *copy* of the beads on a wire will not suffice for our purpose, for this simply replaces one abacus by another (or by a drawing of an abacus) and so constitutes no real development. The essential feature the recording must have is that the result be presented in a form which is *recognisable at a glance*. The problem was solved by arranging the beads in simple patterns. One method of arrangement (which was in part used by the Greeks) is shown in the next line

The weakness of this arrangement, from the psychological point of view, is that the differences between the patterns, particularly the last five, are not sufficiently striking. Another factor which contributed to their falling out of use is their lack of " cursiveness ", which made a rapid recording, on sand or paper, impossible. But, historically, their introduction marks a decisive step forward. In conjunction with the abacus it rendered possible a most valuable abbreviation of a tally and facilitated the recording of large flocks in a simple and easily recognisable form. Here is an illustration of their use. In the first picture we have a tally made on an abacus, and below it is the record of the tally made in a positional notation.

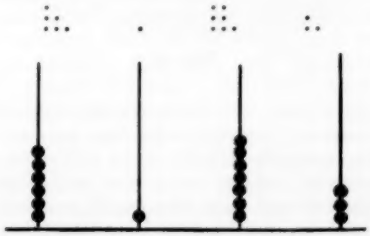


FIG. 1.

Once the use of such number-patterns has been adopted, two obvious lines of development are open. On the one hand, we might give names to the individual patterns, the names then taking the place of the patterns, or we might introduce (in the course of time) some system of symbolic abbreviation of the original patterns. A simultaneous development along both these lines took place (though in part divorced from the positional notation of the abacus). This development was based upon an existing alphabet, with letters in an established order of precedence, and is logically irrelevant. The successive patterns were both called after and represented by the letters in their " natural " order. For the sake of illustration we may take the Roman alphabet as a model, though in fact only older alphabets were actually used in this way. *Translation* from dot patterns to alphabet signs is carried out by means of a table in which the pattern and its letter are placed side by side. For convenience in printing we place the entries in horizontal rather than vertical columns though the latter would be more " natural ".

Thus, for instance, *f a g c* stands for six thousand, one hundred and seventy-three.

The transition from dot patterns to alphabet signs is logically irrelevant, for, as we shall see later, the development of arithmetic does not presuppose the existence of an alphabet, and in fact numbers themselves afford a more useful index of ordinal relations than does an alphabet. None the less the transition is important for reasons which we must now consider.

The dot patterns are not simply an adjunct to an abacus, for once their use has been understood the abacus itself may be dispensed with. Notice first that the construction of the pattern is *progressive*, each pattern being incorporated in the succeeding one, as the following diagram shows.

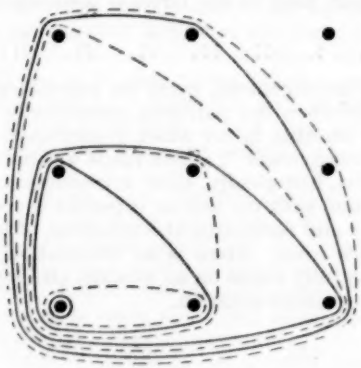


FIG. 2.

Accordingly, in making a tally of a flock, we may place a pebble on the sand for each sheep that passes us, starting with one pebble, then two, and so on, each additional pebble being so placed as to transform the existing pattern into the next. Conversely, taking away one pebble at a time, the pattern may be unmade in such a way that after each removal of a pebble another dot pattern is left, until the last pebble is reached. Making a tally by dot patterns differs, however, in one small respect from using an abacus, or rather

differs from the use of the particular abacus we described. We supposed that each wire of the abacus holds ten beads, whereas only nine can be represented by the foregoing dot patterns, but in fact it would be sufficient for a wire to hold only nine beads, since ten is represented by a single bead on the second wire. The nine-bead abacus is operated by placing the *next* bead on the second wire when the first wire is full and then emptying the first wire. Correspondingly when a flock is being checked against a tally made on a nine-bead abacus a sheep must be matched against the last bead on the second wire *before* the first wire is filled again. In the ten-bead abacus a bead on the second wire *represents* a full first wire, that is to say, may be replaced by a full first wire, but this is not the case with the nine-bead abacus, where a bead on the second wire is the successor, not the representative, of a full first wire. The ten-bead abacus is in fact the simpler to understand, but on account of the dual representation of powers of ten, is logically more complex than the nine-bead abacus. The dot patterns have the same multiplicity as the nine-bead abacus.

Just as the dot patterns may take the place of an abacus, so do the alphabet signs, at first the *names* of the dot patterns, subsequently acquire an independent usage. Instead of *constructing* dot patterns we operate directly with the alphabet signs, their natural order being supposed known and taken for granted. The succession

becomes, simply, *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i* ; as each sheep passes through the gate we say (or write) in turn the alphabet signs, the use of a positional notation extending a relatively small alphabet to an unlimited extent.

In considering positional notations we have not yet mentioned the problem presented by an empty wire. For instance, thirty-one is represented adequately by *ca*, but how shall we represent thirty? If we write just *c* there is no means of knowing whether this stands for three, thirty or even three hundred, since we have not indicated the empty first wire in any way. Looking back at the problem from our present standpoint it seems obvious that all we require is a sign to denote an empty wire—an office which is performed for us by the letter 0—but historically and philosophically this problem was by no means trivial. Remember that the letters are being used to denote dot patterns ; how then, it was asked, can a letter also represent the absence of a dot pattern? (How can nothing be a number?) Yet can we not picture a wire without beads, just as well as a wire on which beads are threaded, and may we not allow some trace of the abacus wire to persist in our new notation? The problem of the zero arises only in passing from an abacus to a symbolic representation of an abacus, and is therefore only a pseudo-problem, a problem in *notation*. The dot patterns, or the alphabet signs, supplemented by the zero, 0, form a complete representation of the abacus.

The transition from dot patterns to alphabet signs takes us from the tally to something fully equivalent to counting. The essential difference between making a tally (for instance, on a single stick), and counting (by alphabet signs) is the difference between longhand and shorthand (almost the difference between travelling and arriving). The tally stick shows not only the flock, but also all the individual parts which make up the flock. Alphabet counting records the flock as a whole, completely obscuring the track by which the total was obtained. The tally stick is like a *copy* of a flock, and the alphabet-count like a *label* on a box, a summary of the contents. Of course, in its primary role of *naming* dot patterns the use of alphabet signs in no way transcends the tally stick, no more than any translation transcends the original ; it is the use of alphabet signs, *per se*, divorced from dot patterns

that constitutes the invention of counting. And this usage, let us repeat, is founded in a supposedly previously established natural order of alphabet signs.

Before turning to the modern system of counting which superseded the use of alphabet signs, there are still a few points of interest to be noticed in connection with the abacus and the positional notation. We have already observed that although our two hands give the number ten an obvious "natural" advantage over any other number as a base of a positional notation, there is no logical reason for preferring one number to another. A large base number, that is, a long-wire abacus, represents large numbers on comparatively few wires, but to offset this advantage renders the transition to dot patterns more difficult, involving as it does the perception of relatively complicated patterns (think of a system of dot patterns for numbers up to twenty). On the other hand, a small base number like two (the base number cannot be less, since an abacus which admits only one bead on each wire is just a primitive tally) whilst reducing the number of dot patterns to a minimum, requires as many as seven wires for the expression of even such a small number as sixty-four. There is, furthermore, no logical necessity for the wires on an abacus to be of equal length, and though an abacus with wires of varying lengths may have no obvious practical advantage, its use is no whit more complicated than that of the more familiar kind, and, what is more important, its possibility helps to break some of our old-established prejudices regarding the nature of a positional notation. For instance, we might construct an abacus in which the first wire carried only one bead, the second only two, the third only three, and so on. Its usage will differ in no respect from the nine-wire abacus we have already described. The following diagram illustrates the representation of the numbers one to nine on this abacus.

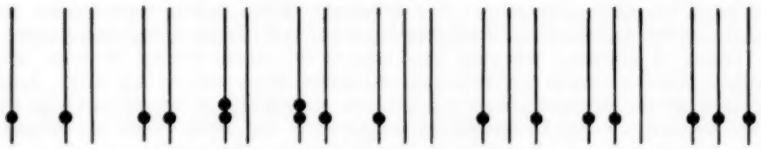
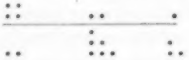


FIG. 3.

A positional notation is by no means the only literal translation of an abacus tally. Apart from the use of various arithmetical operations, which we shall consider later, there are other methods of indicating the abacus wires than the *positions* of the dot patterns, or letters. For instance, we may use the dot patterns twice, both to denote the number of beads on a wire, and also to denote the wire itself. First, wire by wire, we make tallies of the wires in dot patterns (notice that we may treat the wires themselves as a primitive tally) and in this way each wire is associated with a dot pattern, which is in fact a tally of all the wires not on its left. Then we arrange the beads on each wire in a dot pattern placed below the pattern associated with that wire. Thus two thousand and sixty-three is represented by the complex



and ten thousand and forty by

Observe that this system of representation has no need for a sign representing an empty wire, and therefore, occasionally, effects some economy in representation. If we replace the dot patterns by alphabet signs (and distinguish wires and beads by the use of capital and small letters) we obtain the complexes

$$\begin{matrix} D\ B\ A & \qquad & E\ B \\ b\ f\ c & & a\ d \end{matrix}$$

which are practically equivalent to our present representation of numbers *in words*. For though we do not in fact employ letters, the *system* is almost the same ; we call the wires, not *A*, *B*, *C* and so on, but units, tens, hundreds, and so on, and then specify the number of beads on each named wire, ignoring empty wires. The difference between the systems, expressed in modern terminology, is this : in the alphabet notation we number the wires one, two, three and so on, in turn, but in the current notation a wire is called after the number represented on an abacus by placing just a single bead on that wire. The first wire is called the unit, since a single bead on the first wire represents a unit, which is another word for " one ", the second wire is the tens wire since a single bead on the second wire represents the number ten, and so on. That a hundred is ten tens or a thousand ten hundreds is not a theorem (on multiplication), but just an expression of the rule that (on a ten-bead abacus) a bead on one wire may replace or be replaced by a full wire to the right. Interesting questions arise when the number of wires in an abacus exceeds the number of beads which fill a single wire, for the alphabet notation breaks down ; it can, however, readily be extended. For instance, since the alphabet notation, as described above, enables us to record any number up to $10^9 - 1$, that is,

$$\begin{matrix} I\ H\ G\ F\ E\ D\ C\ B\ A \\ i\ i\ i\ i\ i\ i\ i\ i\ i, \end{matrix}$$

by using complexes instead of pairs of letters the notation will cover an abacus of $10^9 - 1$ wires. For instance, $10^{12} + 1$ is represented by

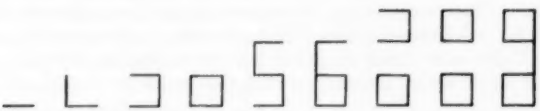
$$\begin{matrix} \begin{pmatrix} B & A \\ a & c \end{pmatrix} & A \\ a & a. \end{matrix}$$

Thus extended, the alphabet notation will represent any number up to

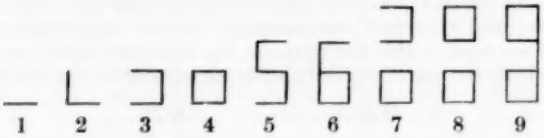
$$k' = 10^k - 1, \text{ where } k = 10^9 - 1.$$

In a similar way, by adding a fourth row, we can next extend the notation to represent any number up to $10^{k'} - 1$, and so on.

The present system of numeration derives not from dot patterns, but from stick patterns. If we make a tally of a collection (of not more than nine objects) by laying sticks in a row, the tally may be rearranged into one and only one of the following patterns :

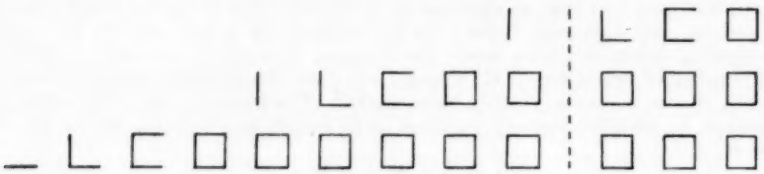


These stick patterns are both more striking, and individual, than the dot patterns, and lend themselves very readily to a cursive reproduction on sand or paper. Compare these patterns with the arabic numerals as we know them today :



Though we have no historical record of these stick patterns the correspondence exhibited in these two rows is itself very strong evidence of a stick pattern ancestry for the present-day designs. Variations are of course possible. It may be that the numeral *three* was at one time reversed, and perhaps also the numeral *nine*. The seven and the nine have lost the lower loop. But none of these changes is more than one would expect to find taking place in the course of a transition from square to cursive patterns.

The stick patterns are not contained one inside the next, which was a remarkable feature of the dot patterns. But this disadvantage is more than offset by the ease with which the patterns can be identified and reproduced. Moreover, these patterns may have descended from an older set, which exhibited this property. For instance, the following :



Here all the signs " look to the right ", and each is contained in the next. They are, however, clearly constructed on the base four, and it is hard to believe that there was a general use of this base antedating the introduction of the base ten.

From each stick pattern we obtain the next by the addition of a stick, and the perception of this is the foundation of counting. By naming the individual patterns (or replacing them by symbolic abbreviations like our numerals), we pass from tally making to counting, as we have already seen in regard to dot patterns. The process of transition may be imagined in a number of stages. First the tally is made by laying sticks in a row, one for each object of the collection of which the tally is being made. Then the complete tally (which we suppose for the moment to be of a collection of fewer than ten things) is arranged in a stick pattern, and the name of this pattern is recorded. This may be regarded as the frontier in the development of arithmetic across which we pass over to counting. So long as we delay the construction of a stick pattern to the *last* stage we have not crossed this frontier. Now suppose that, instead of laying the sticks in a row, we form a stick pattern each time we lay down a stick, the last pattern formed alone being retained. *This is counting*, as we recognise immediately we substitute spoken words for stick operations. Name each pattern as it is formed, and say " and one " for each fresh stick we lay down (and so for each object of the collection) and we shall be accompanying the process of pattern construction by the refrain

one, and one is two, and one is three, and one is four, etc.,

where we have used the English names of numerals as stick pattern names.

Of the two, stick pattern construction and verbal refrain, one is superfluous, for each may stand alone. The one by one passage from each stick

pattern to its sequent may be expressed in words alone, without carrying out the actual pattern construction; instead of laying the stick alongside the three-pattern to make the four, we just say " three and one is four ", and after each " and one " we name the next pattern, without troubling to construct it. In the course of time, we omit the reiterated " and one ", and instead perhaps we touch an object, or just look at it. In this way counting becomes apparently a mere recitation of stick pattern names, one, two, three, four, and so on, which is what makes counting seem at first sight such a mysterious process. But, as we have seen, counting is *not* a recitation, but a process of *transforming a tally into stick patterns*, or, rather, a description of this process without actually carrying out the pattern construction. What in fact we are saying when we count is, let us repeat, " one, and one is two, and one is three, and one is four ", etc., though each " and one " may be expressed otherwise than in words, for instance by moving along the objects we are counting, or even by glancing at them one by one.

With the help of a positional notation (or one of the other devices we have already described in terms of dot patterns or alphabet signs) the extension of counting beyond the range of the nine stick patterns is readily effected. We need only names for the abacus wires (that is for the successive positions occupied by the patterns in a positional notation) and these names are the familiar tens, hundreds, thousands, etc. Counting beyond nine proceeds thus: nine, and one is ten, and one is ten-one, and two is ten-two, ... and one is ten-nine, and one is two-ten, etc. (I have deliberately avoided the contractions thirteen, fourteen, twenty, etc. The forms eleven, twelve refer to a base other than ten, perhaps standing for actual stick patterns of the kinds already referred to above.)

The nomenclature for the successive positions occupied by digits in the positional notation is (apart from the initial positions) capable of systematic extension. The terms units, tens, hundreds, thousands, follow no rule, but are simply specific names for the first four positions. From thousands to millions a serial law operates, giving rise to the terms " tens of thousands " and " hundreds of thousands " (utilising the already prepared term " hundreds " for " ten tens "). From a million, which is a thousand thousands, we advance through ten-million, hundred-million, thousand-million, ten-thousand-million and hundred-thousand-million to a billion (a million-millions). To prolong the nomenclature we require a name for n n's where n is the last name introduced. Thus, just as we have a million for a thousand-thousand, a billion for a million millions, a trillion for a billion billions, then the indefinite prolongation we are seeking may be obtained by adding the ending " illion " to the name of a number $n+1$ to denote n-illion n-illions. For instance, decillion for nonillion nonillions, or million-one-illion for millimillion millimillions. (Since we are here concerned only with possibilities, not with actual usage, we need not attempt to find more melodious substitutes for these cacaphonous hybrids.) Alternative, and simpler, systematic notations are readily devised if we ignore the indications of the current nomenclature. For instance, we may denote the successive positions, in a positional notation, by the addition of some standard termination to the name of the number of the position. In terms of the wires of an abacus, this notation may be described as follows. The first wire we leave unnamed, the next is called the *one-ard*, then comes the *two-ard*, then the *three-ard*, and so on. In this notation a million is a six-ard, and a trillion is a

(two one-ards and four)-ard.

Whatever device is adopted the representation of very large numbers is exceedingly cumbrous. Refinements of notation increase the number of

numbers susceptible of a brief representation, but inevitably numbers are reached too great for single-fold expression.

Important though the invention of a scale of notation has been in the development of arithmetic, this device tends to obscure the fundamental character of the number concept, for the kernel of the concept lies in the primitive tally. Let us suppose we make a primitive tally in marks on a strip of paper, for instance, for a flock of a dozen sheep, thus :

1 1 1 1 1 1 1 1 1 1 1 1.

The notation is adequate provided the sign complex is regarded as a whole. Ambiguity arises, however, if we ask ourselves whether this tally records a *single* flock or not. There is nothing in this form to distinguish between the tally of a flock of a dozen sheep and the tally of, say, one half dozen, placed alongside the tally of another half dozen. From this point of view, the notation is not sufficiently closely knit, fails to delimit the bounds of the sign and fails to combine its parts into a single whole. To cement the several parts of the sign, one to another, we introduce a symbolic abbreviation for the word " and ", which we write " + " (the sign itself is a corruption of the initial letter of the Latin *plus*), and insert it between each pair of consecutive marks. The resulting sign takes the form

$$1+1+1+1+1+1+1+1+1+1+1+1,$$

which is now free from ambiguity. The sign is read as " one and one and one . . . and one ". However, another objection arises now, namely that the sign " 1 " is being used in two different ways. Each " 1 " after the first is prefixed by the sign " + ", but the first " 1 " has no prefix. If we simply write another " + " in front of the first " 1 ", the whole value of using the " + " is lost, since the sign complex is again rendered ambiguous (compare $+1+1+1+1+1$ with $+1+1\ \ +1+1+1$). The simplest way to maintain the force of the + sign, and to limit the use of the unit sign to the single " +1 " is to start the row of units with a non-significant commencement sign, a sign which by itself denotes, not a tally of a flock, but an absence of objects, like an abacus wire empty of beads, or an empty sheep pen. The zero, which we have already used to denote an empty wire, now serves also as a commencement sign for a tally, and the tally of a dozen becomes

$$0+1+1+1+1+1+1+1+1+1+1+1+1.$$

This sign is constructed entirely from the commencement sign " 0 " and the iteration of " + 1 ", its beginning and end are clearly delimited and its several parts are united in a comprehensive whole. If we characterise such a sign, for the moment, as a numeral pattern, then we perceive that numeral patterns have that property of being contained one inside another to which we have already drawn attention in relation to dot patterns and prototype stick patterns. This is exhibited in the following diagram by the use of brackets :

$$(((((0+1)+1)+1)+1)+1).$$

Each sign complex between brackets is (ignoring the interior brackets) a numeral pattern. From any numeral pattern another is constructed by affixing a terminal " +1 ", and accordingly the process of numeral pattern construction is *without end*.

To formulate a general definition of numeral patterns we introduce some arbitrary letter, x (or whatever distinguishing mark we please), and define the numeral patterns as the sign complexes obtained from the sign " x " after eliminating " x " by either the operation of replacing " x " by " $x+1$ " or replacing " x " by " 0 ". Thus, for instance, the pattern $0+1+1+1$ is

obtained after four operations, deriving in turn x, $x+1$, $x+1+1$, $x+1+1+1$, $0+1+1+1$; here we replace "x" by "$x+1$" three times in succession, and then replace "x" by "0". This definition in effect says that a numeral pattern is either "0" or the sign obtained by affixing "+1" to a numeral pattern. The letter x introduced above is a symbol for a numeral pattern, and is called a *numeral variable*. The usage of a numeral variable x is determined by the rule that "x" may be replaced by "0" or "$x+1$".

The transformation of numeral patterns into conventional numerals is effected by a process akin to counting. We define in turn the four numerals 1, 2, 3, 4, etc., by the formal equivalences.

1 is $0+1$, 2 is $1+1$, 3 is $2+1$, 4 is $3+1$, etc.; by means of these equivalents we may transform any numeral pattern into a conventional numeral; for instance

$$\begin{aligned} &0+1+1+1+1+1+1+1 \\ \text{transforms first into} \quad &1+1+1+1+1+1+1+1 \\ \text{then to} \quad &2+1+1+1+1+1+1 \\ \text{and then in turn to} \quad &3+1+1+1+1+1 \\ &4+1+1+1+1 \\ &5+1+1 \\ &6+1 \\ &7 \end{aligned}$$

The transformation process amounts to enclosing the successive numeral patterns, contained in the composite pattern, between brackets, and then starting from the innermost bracket, replacing, in turn, $0+1$ by 1, $1+1$ by 2, $2+1$ by 3, and so on. This is exhibited in the following diagram, by deleting in turn the pattern replaced.

$$\begin{array}{c} \mathit{1}\quad\mathit{2}\quad\mathit{3}\quad\mathit{4}\quad\mathit{5}\quad\mathit{6}\quad\mathit{7} \\ (((((((\emptyset+\mathit{1})+\mathit{1})+\mathit{1})+\mathit{1})+\mathit{1})+\mathit{1})+\mathit{1}). \end{array}$$

A better illustration could be given in sand, by rubbing out each pattern when it is replaced by another. The point we are seeking to emphasise is that the transformation process is of a serial character, and operates like an automatic counting machine. The repeated copying of the "tail" of the numeral pattern, shown in the first diagram, is no part of the process itself, and is a feature only of the method of illustration.

Using the familiar equality sign "=" between symbols to denote their equivalence, that is to say, to express the fact that one symbol may be replaced by the other, the foregoing equivalences are expressed by the formulae

$$1=0+1,\ 2=1+1,\ 3=2+1,\ 4=3+1,\ \text{etc.}$$

These formulae are the foundation of arithmetic, and the *conventional* use of number-signs which they exemplify is typical of the whole of arithmetic. The formulae are valid in the same sense that a dictionary is valid, each entry in the dictionary expressing the equivalence of two words, or phrases, this equivalence being established only by custom and convention. The use of number signs in language, as opposed to the use in arithmetic, is of an entirely different character; if I say that there are three matches in my pocket, then what I say is true or false, not solely in virtue of the conventions that $1+1=2$, $2+1=3$, but in virtue of what matches I actually have in my pocket. A fact, not a convention, is now in question. That it is the same thing to say I have three matches, as to say I have two and one matches is a consequence of the *convention* $2+1=3$, but this does not decide the truth of the proposition, whichever sentence it is expressed by. The question is simply "Do the

matches in my pocket form a three-sign or not? " And this is the same as asking such a question as " Is there a question mark on this page? " And the test for this is the empirical one of observation. No convention of arithmetic is a substitute for experiment.

To emphasise the correspondence between counting and transforming numeral patterns, consider the question " How many *a*'s are there in the following line? "

a a a a a a a

If we recall the steps by which the numbering of a flock is effected, we see that two processes are involved. First the group to be counted is transformed into a numeral pattern by *substituting* for each element of the group the sign + 1, and then the resulting numeral pattern is transformed into a conventional numeral (or of course we may employ dot or stick patterns instead). Both these processes are illustrated in the following diagram, which gives a schematic representation of the process of counting the line of *a*'s :

1 2 3 4 5 6 7
0 + 1 + 1 + 1 + 1 + 1 + 1 + 1
a a a a a a a

We have indicated the substitution of one sign for another by deleting the sign which has been replaced. The operations are carried out from left to right, new signs being written above, instead of in place of, the signs they replace.

Comparing this diagram with the foregoing it is evident that the only difference between counting objects and transforming a numeral pattern lies in the initial step of replacing each object by " + 1 ". Making a tally, as opposed to counting, stops short at this initial step. We shall call the process of replacing each object of a group by the sign " + 1 " (actually or just in imagination) *regarding the group as a number sign*, so that we may say that making a tally consists only in regarding a group as a number sign, and counting consists in transforming a group, regarded as a number sign, into a conventional numeral. *Counting is not a process of discovery, but of transformation.*

Except in the case of bad handwriting or imperfect printing, the question whether, for instance, a certain sign is a letter *a* or not, simply does not arise. There is no unique pattern to which a sign must conform to be a letter *a*, but the accepted range of variation of signs which are recognised as *a*'s is quite small, and throughout the range of variation the signs are recognisably different from any in the range of variation of another letter. In fact, although there is no standard *a* comparable with the standard yard, the various *a* signs may be regarded as copies of a prototype *a*, just as the signs which a child makes are copies of these which his teacher sets before him.

This matter of copying a sign is of fundamental importance, in arithmetic as in language. The relationship between a copy and that of which it is a copy is by no means an obvious one. Perhaps the simplest and most direct copy is a *tracing*, for what is called a free-hand copy, involving as it does the coordination of hand and eye, leaves open the question of the *method* of copying ; but even a free-hand copy may be matched against the original, perhaps by superposition, or even by means of a double tracing, so the test of the copy, as a *copy*, may be held to lie in this matching, independently of the way in which the copy is obtained. In this sense a poem recited by heart is a copy of the poem learnt, despite the unknown mechanism which connects the two, and so too a written letter *a* is a copy even though there is no *a*, or image of an *a*, before us when we write it. It cannot be essential that, when we write a letter or recite a poem, we have an image before our mind's eye, for such an image could only be the product of some habit of the imagina-

tion and therefore of the same kind as a habit of the hand or tongue. This shows that what constitutes a copy is not the method by which it is obtained, but the test by which the copy is matched against the original. (We are not here concerned with such questions as the infringement of copyright, where the *method* of copying is the very point at issue.)

In arithmetic the problem of checking a copy is two-fold. First there is the matching of simple signs, like one " 3 " with another " 3 ", and this is exactly the same as matching one letter with another. But in addition we have the more elaborate problem of matching two complexes of signs, and this may often be done in two different ways. Consider for example these two numeral patterns :

$$0+1+1+1+1+1+1+1 \qquad 0+1+1+1+1+1+1+1.$$

Are these the same pattern or not? If we transform each to a conventional numeral, we obtain the same numeral each time, namely 7. Alternatively we may match the signs one against the other, either by direct superposition, or by crossing off from the left, or even by conjoining the signs by a series of lines. It does not matter which method of matching we accept, so long as *some* method of deciding whether or not the signs are the same is laid down. Different methods of matching may not give the same result (even such methods as we have just described) and this possibility gives rise to an interesting question which we shall consider shortly.

Making a tally, which we have called regarding a group as a number sign, is the same operation as copying, or matching, a complex sign.

We have followed the development of the notations of arithmetic from the primitive tally stick to the cursive numerals of current usage, and we turn now to consider the antithesis expressed by the words *numeral* and *number*.

The terms *numeral* and *number-sign* are synonymous, so that the antithesis under consideration is between " number " and " number-sign ". Number-signs are not simply what we have called conventional numerals, but any of the signs we have described from a simple tally stick to a dot or stick pattern complex in positional notation. But if every sign we have so far described is a number-sign, what are the numbers of which these are *signs*? We answered the question " What is the number of sheep in this flock? " by constructing a number sign. Does this mean that number is just another word for numeral and that *number-sign* is a nonsensical term? Certainly there appears to be nothing apart from the numerals to which we can point when we try to answer the question " What is a number? " Words like " table " and " chair " are tokens which we can exchange for actual tables and chairs, but the word " number ", looked at in this way, seems to be a base currency. Yet if we try to identify numeral and number we meet an insuperable difficulty in the multiplicity of number signs. Which of the many three-signs, for instance, is the number three? Is it 1 1 1 or $0+1+1+1$ or $\therefore$ or 3? And why not x, y, z or a trio of sheep? Of course we may seek for a way out of this difficulty by saying that the number three is not any one of these signs, but is the totality of them. Superficially this answer seems to be adequate, but further consideration tends to weaken its force. When we talk of the totality of three-signs, presumably we mean not only the totality of three-signs hitherto written down or perceived, but must include also every future instance of a three and every symbolic representation of a three that may be conceived of. Thus the totality is not some definite group of things, something complete in itself; it must therefore be specified, not by an enumeration, but by the *property* which its members have in common. But if we identify a number with a totality defined only by the common property of its members, then we are in effect saying, not that a number is a totality of

number-signs, but that number *is* this common property. And the only thing which, for example, the three-signs have in common, is their *common usage* in arithmetic as copies or transforms of the numeral pattern $0+1+1+1$.

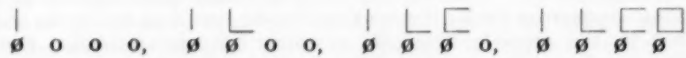
Accordingly, a number is not a numeral, nor a collection of numerals, but is the role played in arithmetic by a numeral, that is, the role taken by a sign when it is used as a copy or a transform of a numeral pattern.

An analogy may help to make this clearer. Suppose we ask "Who is Shakespeare's Hamlet?" (assuming, as is in fact the case, that Hamlet is not the name of a former Prince of Denmark). As with a number and the signs that denote it, Hamlet is not the actor who plays the part, nor the totality of such actors, but what they all have in common, namely the *role* which they are playing, and it is this role which is Hamlet.

Number signs have many uses in language, though their use in counting is perhaps the most important. Next to counting, the commonest service they perform is the representation of *rank* or *order*, as for instance when we speak of the *fourth* day of the month or the *seventy-first* year of a man's life. The process of numeral pattern construction is a *serial* process in which each pattern has by definition a successor, the successor of any pattern x being the pattern $x+1$. Starting with the pattern 0 we construct *in turn* the patterns $0+1$, $0+1+1$, $0+1+1+1$, and so on. Each sign constructed is a *proper part* of its successor, so that the process never leads back to a sign previously constructed, and this is what we mean when we say that there is an *infinity* of numbers (the term infinity denoting simply *without end*). It may well be that in *practice* there is a limit to the numbers we may write down, since, for instance, the very mass in ink and paper of the numbers constructed, if the process were carried on long enough, would eventually exceed the mass of the earth and destroy every particle of life upon it. All that is implied in saying that there is an infinity of numbers is that no limit is laid down to the repetition of the number construction process. It is the *possibility* of constructing numeral patterns that is without end, not the actual construction process.

Since the numeral patterns do not form a closed cycle, no numeral is amongst its own successors, and so numeral patterns (or their abbreviations) supply an endless source of labels by which we can distinguish the elements of a group however great. For comparatively small groups, alphabet letters serve as well as numerals for labels, but the alphabet letters have an established order only in virtue of some accepted standard list, whereas the order of the numerals derives from the very process of generation, and if the numerals are expressed in a sufficiently condensed notation this order is immediately perceptible. (It is almost certain, moreover, that our established order for the letters of an alphabet is a much later innovation than ordering by numbers.)

The difference between counting and ordering by numbers, from the standpoint of stick patterns, is that in counting the stick patterns are *transformed* at each stage into the succeeding pattern, no record of the pattern transformed being kept, the final pattern obtained representing the number counted, whereas in ordering by numbers each pattern constructed is retained and is called the *ordinal* number of the last object counted. To take a concrete example, if we count the rows of hoops o o o o by stick patterns all that we retain is the single pattern □, but if we seek to order the hoops we obtain in turn



and the final configuration is retained *in toto*, each stick pattern representing

the ordinal number of the hoop beneath it. In terms of conventional numerals the difference is between

1 2 3 4 and 1 2 3 4

ø ø ø ø ø ø ø ø

which brings to light the important fact that the very process of counting establishes the objects counted in (serial) order. In counting we *ignore* (that is, overlook) the ordering by numbers which takes place on the way, and so there is a sense in which the ordinal use of numerals is more fundamental than their use in counting; in spite of this the number of a group is called a *cardinal* number.

The cardinal number of a group represents the group *as a whole*, but ordinal numbers serve to *distinguish the elements*. When a line of soldiers "numbers off", each soldier calls his ordinal number and the ordinal number of the last soldier is also the cardinal number of the line. Moreover the ordinal number of any soldier is also the cardinal number of the section of the line which that soldier terminates. Thus the *cardinal* number of a group is also the ordinal number of an element of the group and is amongst the successors of the ordinal number of every other element.

Another way in which we can bring out the difference between cardinal and ordinal numbers is by observing that the ordinal number of an element X of a group is the successor of the cardinal of the subgroup of all the elements counted before X. Thus a man is ninth in a line if he follows a group of eight men. Observe that the man's ordinal number is independent of the actual order of the men in front of him (and of the men after him), and depends only upon the *cardinal* number of the group that precedes him. The terms "precede" and "succeed" which we have used in relation to the elements of a group refer to the order established by the process of counting or making a tally, elements already counted at some stage in the process being said to precede the elements not yet counted, and the latter being said to follow or succeed the former. We have already observed that counting necessarily establishes an order among the objects counted, namely the order of selection. To count a field of sheep, the sheep must be selected one after the other, and some device adopted for distinguishing those already counted from the rest; this may be done by passing the flock through a gate, or marking each one counted. We could also label each sheep with its ordinal, that is, the number which succeeds that on the last label attached, or, what is the same thing, the number which succeeds the cardinal of the group so far counted. But if our object is to guard against loss from the flock the use of ordinal number is no added security, for even if we find sheep number eleven, sheep number three may be missing.

The elements of a (finite) group may be said to have an *order* if, and only if, each element, save one alone, has a unique successor, and each, save one alone, a unique predecessor. In such a case as men in a line the relations of *left hand* and *right hand* serve to determine each man's successor and predecessor, but for a general group the determination of predecessors and successors involves an *ad hoc* act of choice. If it is (physically) impossible to make such a choice (as, for instance, when the elements of a group are in rapid motion) then ordering is impossible—and so, too, is counting. The elements of a group may or may not admit a unique order in relation to some attribute of the elements. For instance, a line of men may be arranged in descending order of heights provided no two are of the same height, but if there are two of equal height, the ordering is not complete. It is important to remember that such an ordering (when it is unique) may be attained with-

out the use of numbers. It is not at all necessary that the various heights be expressible in multiples of some unit ; provided we can say of any two men of the group which is the taller (by placing them back to back, for instance), the whole line may be arranged in descending order of heights. We start with any two, and settle their order. A third is then put in relation to the first two, simply by comparing him with each of them (he will stand in front if taller than the first, at the back if the second is taller than he, and between the two otherwise). A fourth is then brought into place, by fixing his position with regard to each of the first three, and so on.

The words *first, second, third, fourth,* and so on, stand for the attributes *numbered one, numbered two, numbered three, numbered four,* etc., that is to say, the first day is that which bears the number one, the second house is that which bears the number two, and so on. By " bears the number ", however, we mean a variety of things. The house *bears* its number on the door, a day *bears* its date number in the calendar, and the third soldier in the line *bears* the number three only in the sense that he called himself *three* when the line numbered off. When we say that, for instance, seven is the seventh number, we say no more than that the number seven is the number seven. This is bound up with the common fallacy that it is significant to say that there are six numbers from one to six ; for if we counted the numbers one to six we should merely write down (or say) the numbers one to six twice over. The numbers one to six are, so to speak, the standard by which we judge a group to have six members, and it no more makes sense to say that there are six numbers from one to six than to say that the standard yard is one yard long. If we see a row of houses numbered from one to six, then the row has *already been counted* for us.

In focusing attention on the need to order the elements of a group which is being counted, we bring to mind the question of the dependence of the cardinal number of a group upon the order of its elements. How do we know that changing the order will not change the cardinal of the group? This is, in fact, no more than a part of the wider question : if we count the elements of a group more than once (in the same order or not) will we necessarily obtain the same cardinal each time? Certainly, as we all know, counting and recounting large groups often leads to conflicting results, and the usual supposition is that in some of the counts we have made a mistake, and that there could really be only one correct answer. The problem, however, is deeper than this leads one to suppose. Sometimes we do find that we have made a mistake ; we *remember* saying " seven and one is nine " at some point in the counting, or we notice an element previously overlooked, or an element counted twice. But if we *cannot* find a mistake, are we forced to concede a mistake entirely on the strength of the divergence of the answers? In practice we deal with a situation of this kind in a variety of ways. We establish various precautions to guard against error, such as having independent witnesses to watch that no element is added or removed during the counting, and arranging for several people to take part in the recounts. Men are classified as good or bad counters according to the frequency with which they obtain the same answer, and so on. Yet it might happen that, despite every precaution, in counting and recounting a particular group we continue to obtain a variety of answers, or perhaps two answers at alternate counts, like six million and three alternating with six million and four. Such a group might of course be dismissed as *uncountable*, and various hypotheses formulated to account for the mystery (such as elements coalescing and separating on alternate counts). Or we might accept the disjunction *six million and three or four* as the answer, or to select the lesser or greater according to circumstances. Another possibility would be to accept the two answers as evidence of two facts, like a drawing which may be looked at in two ways (the so-called optical

illusion). To say that there *must* be some one definite answer, whether we can find it by counting or not, says nothing at all, for, by definition, the number of a group is found by counting.

A group yielding different cardinals on successive counts is, to draw an extreme comparison, like a stick pattern changing from four to five before our eyes despite every precaution against human interference or fraud. If such occurrences were the commonplace, numbers would entirely lose their practical value.

We are so familiar with the use of numbers in language, in our everyday life, that we are too readily apt to suppose that we could not dispense with them without an intolerable sacrifice of our means to describe the world as we see it. There is no doubt that without numbers language would be exceedingly cumbrous and complicated, but a language lacking number words can nevertheless express everything that can be said in the current medium of expression. That is the meaning of the analysis of number which we have given above. Numerals are *abbreviations* and every abbreviation is dispensable. The route which leads from a group to its cardinal (or to the ordinal of a term of the group) is traversable *both ways* and everything we express by means of numbers may be expanded in sentences from which the number words have been eliminated. Since *a car and a car and a car* is three cars, the sentence " I bought three cars this year " says only that " I bought a car and a car and a car this year ", and there are no number words in this latter sentence. You may say if you like that *a car and a car and a car* is an instance of the number three, but this simply means that if you establish an arithmetic *then* you regain your number words ; we are under no necessity to do so, other than the urge for brevity. From this point of view one may in fact doubt that arithmetic has any value. If " three cars " is just another way of saying " a car and a car and a car ", why do we bother to introduce this new term *three*, or, to use a mathematical expression, why do we introduce a *new notation*? And the answer is that new notations are amongst the finest and most valuable discoveries of which the human mind is capable, for a notation is something which does our work for us. Try and multiply three hundred and sixty-four by six hundred and seventy-two, first on an abacus and then in our modern notation and see how much work the notation does for you.

The elimination of number words, whilst necessarily possible, is not always so straightforward as the previous example suggests, owing to the maturity and flexibility of our language and to the vast range of services which numbers perform. Ordinals, however, present little more difficulty than cardinals. If I say, for instance, that I bought my third car this year, I can eliminate the ordinal *third* by saying instead that I bought a car after I had bought a car and a car this year.

The *adverbial* use of number exemplified by the words once, twice, thrice, etc., is equally easy to understand provided we remember that adverbs qualify phrases and not just nouns. Thus " he paid his fare twice " means " he paid his fare and he paid his fare " and *not* " he paid his fare and his fare " (which is " he paid two fares ").

The terms single, double, treble, etc., are used both as adjectives and verbs. The adjectival use, is, with certain exceptions, purely a literary variant of the ordinary cardinals. To pay double fare is to pay two fares, and a double-deck bus is a two-deck bus. But a double bed is one bed for two persons. " Double " used as a verb is often particularly difficult to eliminate. " Careful driving doubles the life of a tyre " may perhaps be rendered " Careful driving makes a tyre last a life and a life ", where we have first transformed the verb " to double the life " into the adjective " *double* life ". R. L. G.

MATHEMATICAL NOTES

2597. *The roots of a transcendental equation.*

In a recent paper (*Gazette*, Sept. 1954, p. 161) H. Goldenberg has proved that the equation

$$F(z) \equiv \coth z + b - c/z = 0, \quad \left.\begin{matrix} c < 1 \\ b > 0 \end{matrix}\right\}$$

has no roots with positive real part. This result can be proved very simply, in the following way.

We have, using the familiar expansion of coth z,

$$F(z) = b + (1-c)/z + \sum_{n=1}^{\infty} \left(\frac{1}{z - n\pi i} + \frac{1}{z + n\pi i} \right).$$

Now if $\mathscr{R}z > 0$, $\mathscr{R}(z \pm n\pi i) > 0$, whence $\mathscr{R}\dfrac{1}{z \pm n\pi i} > 0$, for each n. Thus if $\mathscr{R}z > 0$, $\mathscr{R}F > 0$, and the required result follows.

It is, incidentally, immediately obvious from this proof why the conditions $c < 1$, $b > 0$ arise, and that, moreover, equality is permissible in both cases.

It may be remarked that the above is only one of many miscellaneous instances in which " positive " functions (i.e. functions whose real part is positive in the right half-plane of the variable) may be usefully invoked. A paper on properties and applications of such functions is in course of preparation. The properties of positive functions are of particular importance in the theory of electric networks, and analogous systems, since the impedance function of a general two-terminal network is necessarily positive.

A. TALBOT.

2598. *The addition theorems for circular and hyperbolic sine.*

Since for small x and y, $\sin(x+y) = \sin x + \sin y$, we try

$$\sin(x+y) = f(y)\sin x + f(x)\sin y,$$

or, more generally, we look for functions $\phi(x)$ with an addition theorem of the type

$$\phi(x+y) = f(x)\phi(y) + f(y)\phi(x). \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(A)}$$

Equation (A) was solved by Abel in 1823 as an example in his very elegant paper* on functional equations. Abel's method is substantially as follows; put $x + y = c$ and differentiate with respect to x, so that

$$0 = f'(x)\phi(y) - f(x)\phi'(y) - f'(y)\phi(x) + f(y)\phi'(x).$$

Giving y any two constant values, for example, $y = 0$, $y = 1$, we have two differential equations of the form

$$\alpha\phi'(x) + \beta\phi(x) + \gamma f'(x) + \delta f(x) = 0,$$

$$\alpha'\phi'(x) + \beta'\phi(x) + \gamma' f'(x) + \delta' f(x) = 0,$$

whose solutions are of the form

$$\phi(x) = Ae^{mx} + Be^{nx},$$

$$f(x) = \lambda Ae^{mx} + \mu Be^{nx}.$$

If we substitute these solutions in the right-hand side of (A), we have

$$\phi(x+y) = 2A^2\lambda e^{m(x+y)} + 2B^2\mu e^{n(x+y)} + (\lambda+\mu)AB(e^{mx+ny} + e^{nx+my}),$$

* *Oeuvres* (1881), t.I, p. 10.

whence

$$2A^2\lambda = A, \quad 2B^2\mu = B, (\lambda+\mu)AB = 0.$$

We can suppose A and B are both non-zero ; then

$$A\lambda = B\mu = \tfrac{1}{2}, \quad A = -B,$$

that is,

$$\phi(x) = A(e^{mx} - e^{nx}), \quad f(x) = \tfrac{1}{2}(e^{mx} + e^{nx});$$

putting $m = n = i$, $A = -\frac{1}{2}i$; and $m = -n = 1$, $A = \frac{1}{2}$ give respectively

$$\phi(x) = \sin x, \quad \phi(x) = \sinh x.$$

J. C. W. D. la BERE

2599. *An inequality for Euler's function $\phi(n)$.*

The number $\phi(n)$ of numbers less than and prime to n satisfies the inequality

$$a^{\phi(n)} > an$$

for $a > 6$ and $n > 2$.

We consider first the case $n = 2^p$, $p \geqq 2$.

Since $a^2 > 4a$, if $a > 4$, therefore $a^4 > 16a^2 > 8a$, $a^8 > 64a^2 > 16a$ and so on, up to $a^{\phi(n)} > 2^p a = an$.

We take next the case when n is odd and greater than unity ; for this case we prove the stronger result

$$a^{\phi(n)} > 2an.$$

For if p is an odd prime and $a > 6$, then

$$a^{p-2} > 2^{p-2} \cdot 3^{p-2} \geqq 2(2+1)^{p-2} \leqq 2(2+p-2) = 2p$$

and so $a^{p-1} > 2ap$ whence $a^{(p-1)(q-1)} > 2p\,a^{q-1} > 2apq$, $a^{(p-1)(q-1)(r-1)} > 2apqr$ and so on. Hence if

$$n = p^{\alpha+1} \cdot q^{\beta+1} \cdot r^{\gamma+1} \ldots = kpqr \ldots, \text{ say,}$$

then $$\phi(n) = k(p-1)(q-1)(r-1) \ldots$$

and $$a^{\phi(n)} > 2a(k+1)pqr \ldots > 2an.$$

If $n = 2^p M$ where $p \geqq 2$ and M is odd and greater than unity then

$$\phi(n) = \phi(2^p)\phi(M) = 2^{p-1}\phi(M)$$

and so, since,

$$a^{\phi(M)} > aM$$

therefore

$$a^{\phi(n)} > Ma^{\phi(2^p)} > 2^p Ma = an.$$

Finally, if $n = 2M$ where M is an odd prime greater than unity then

$$a^{\phi(n)} = a^{\phi(M)} > 2aM = an,$$

which completes the proof.

R. L. GOODSTEIN

2600. *Digit transfers.*

The method used by Professor Littlewood in Note 2494 for the particular case $p/q = 3/2$ provides a simpler answer to the question for what values of p/q there exists

$$x = 10^n a_n + 10^{n-1}a_{n-1} + \ \ldots\ + 10a_1 + a_0$$

such that $$10^n a_{n-1} + 10^{n-1}a_{n-2} + \ \ldots\ + 10a_0 + a_n = px/q,$$

than is given in Note 2495.

For if $$\xi = a_n \cdot \dot{a}_{n-1}a_{n-2} \ldots a_0\dot{a}_n$$

then $$\xi - a_n = \frac{p}{q} \cdot \frac{\xi}{10}$$

so that $$\xi = \frac{10qa_n}{10q-p}.$$

If $10q-p$ may be expressed as a product Nr where $r<10$ and N is prime to 10, then *provided that* $p<N$, so that $r<10q/N<(r+1)$ and therefore $q<N$, it follows that q/N is a pure recurring decimal, the decimal part of ξ, the whole part of ξ being $a_n=r$. If however $10q-p$ cannot be expressed as a product Nr with $r<10$ and N prime to 10, then $qa_n/(10q-p)$ is not a pure recurring decimal and there is no solution. R. L. G.

2601. *A matrix transformation.*

$[A_{ijk}]$, i, j, k running from 1 to n, is a set of n square arrays

$$A_{ij1},\quad A_{ij2},\ \ldots,\ A_{ijn}.$$

T is the transformation which carries $[A_{ijk}]$ into n arrays $[B_{ijk}]$ where the rth column of the sth matrix is carried into the sth row of the rth matrix, so that, for each r and s from 1 to n,

$$A_{irs} = B_{sir},\quad i = 1, 2, \ldots, n.$$

Thus $T[A_{ijk}]=[A_{kij}]$ and therefore $T^3[A_{ijk}]=[A_{ijk}]$. $U[A_{ijk}]$ is the set of matrices obtained by omitting from $T[A_{ijk}]$ any row which is a row in $[A_{ijk}]$.

We show that $[A_{ijk}]$ may be chosen so that $U[A_{ijk}]$ is a single given square matrix.

For the sake of exposition we consider the case $n=4$, but the method is perfectly general.

Let $A_{ij1}=a_{ij}$, where a_{ij} are arbitrary. Then we define in turn

$$\begin{array}{llll} A_{1j2}=a_{j2}, & A_{2j2}=a_{j3}, & A_{3j2}=a_{j4}, & A_{4j2}=b_j \\ A_{1j3}=A_{j22}, & A_{2j3}=A_{j32}, & A_{3j3}=A_{j42}, & A_{4j3}=c_j \\ A_{1j4}=A_{j23}, & A_{2j4}=A_{j33}, & A_{3j4}=A_{j43}, & A_{4j4}=d_j. \end{array}$$

The construction ensures that the 2nd, 3rd and 4th columns in the first three matrices are rows in $[A_{ijk}]$. We now identify the 2nd, 3rd and 4th columns in the 4th matrix with the 4th rows in the 2nd and 3rd matrices and the 1st row in the 1st matrix respectively, giving

$$\begin{array}{lll} a_{33}=b_1, & a_{43}=c_1 & c_2=a_{11} \\ a_{34}=b_2, & a_{44}=c_2 & c_3=a_{12} \\ d_3=b_4, & b_4=c_3 & c_4=a_{13} \\ & d_3=c_4 & d_4=a_{14}. \end{array}$$

which may be satisfied by taking

$$\begin{array}{llll} b_1=a_{33}, & b_2=a_{34}, & c_2=a_{44}=a_{11}, & c_1=a_{43} \\ b_4=c_3=d_3=a_{13}, & & c_4=d_3=a_{13}, & d_4=a_{14}. \end{array}$$

Then $U[A_{ijk}]$ is the single matrix

$$\begin{array}{cccc} a_{11} & a_{21} & a_{31} & a_{41} \\ a_{12} & a_{13} & a_{14} & a_{23} \\ a_{22} & a_{32} & a_{42} & a_{43} \\ a_{23} & a_{24} & a_{34} & d_1 \end{array}$$

each of whose elements is unrestricted.

Since the operation T is reversible, by applying T^{-1} to the foregoing $[A_{ijk}]$ we determine $[B_{ijk}]$ such that $UT[B_{ijk}]$ is an assigned single matrix.

PETER GOODSTEIN

2602. *Five-interval and seven-interval formulae for numerical integration.*

In Note 2489 (February 1955) Mr. R. Hamilton Dick gives formulae for numerical integration over five and seven intervals stated to be "exact for functions whose second order differences are constant", but actually exact provided that third order differences are constant.

It is a little difficult to understand the purpose of evolving special multi-point formulae, restricted to constant third order differences, since any odd number, $2n+3$, of intervals can be covered by n applications of Simpson's rule with one application of the Three-eighths rule. It is true that Mr. Hamilton Dick's five-interval rule has a rather smaller remainder term than Simpson + Three-eighths, but his seven-interval rule has a larger remainder term than twice Simpson + Three-eighths.

Even if third differences are constant, it is little more difficult to use Gregory's formula than either of the above methods. Admittedly the remainder term, if second difference terms only are used, is larger than with the other methods. One can, however use as many orders of differences as may be necessary and Gregory's formula is therefore capable of greater accuracy in cases where the third order differences are not constant, as for instance the example chosen by Mr. Hamilton Dick. There is much to be said for using Gregory's formula always, as then one has one rule to cover all cases.

It may be convenient to quote the formulae referred to above;
Simpson (for two intervals)

$$I = \tfrac{1}{3}w(y_0 + 4y_1 + y_2),$$

Three-eighths (for three intervals)

$$I = \tfrac{3}{8}w(y_0 + 3y_1 + 3y_2 + y_3),$$

Gregory (for n intervals)

$$\begin{aligned} I = w\{&\tfrac{1}{2}y_0 + y_1 + y_2 + \ \dots\ + y_{n-1} + \tfrac{1}{2}y_n \\ &- \tfrac{1}{12}(\delta y_{n-\frac{1}{2}} - \delta y_{\frac{1}{2}}) - \tfrac{1}{24}(\delta^2 y_{n-1} + \delta^2 y_1) \\ &- \tfrac{19}{720}(\delta^3 y_{n-1\frac{1}{2}} - \delta^3 y_{1\frac{1}{2}}) - \tfrac{3}{160}(\delta^4 y_{n-2} + \delta^4 y_2) - \dots\} \end{aligned}$$

where δ is the central difference operator.

A. N. Black

2603. *Dr. Beddoes and Benjamin Donne,* on the teaching of Mathematics.*

In *The Mathematical Gazette* for December 1954, Miss D. M. Milton has some interesting remarks to make on Dr. Thomas Beddoes's views on the teaching of mathematics. She demonstrates that Beddoes rebelled against "the argumentative and abstract methods of his own day" and in particular recommended the use of models in solid geometry. Beddoes was of course the son in law of R. L. Edgeworth and the brother-in-law of Maria Edgeworth, authors of the very important work, *Practical Education*. He was also a friend of most members of the Lunar Society in Birmingham and therefore well acquainted with the educational views of Joseph Priestley, Erasmus Darwin, Josiah Wedgwood and others. With all that Miss Milton has to say about the importance of Beddoes's ideas on mathematics teaching I am therefore in agreement.

It is unfortunate, however, that Miss Milton chooses to illustrate the opposite of Beddoes's ideas and what she calls "the accepted teaching of mathematics

* I am grateful to Dr. J. E. Tongue, Librarian of the Bristol Baptist College, for lending me the college's copy of Benjamin Donne's *Essay on Mechanical Geometry*. The college was one of the first subscribers to the book in 1796 and also employed Donne and his son as lecturers to its own students.

in 1769 " from a work by Benjamin Donn. The quotation, I admit is frightening and after reading it one would be ready to consign Donn to the hell of being eternally a pupil in his own classroom. I do not feel, however, that this would be at all just to him. In 1796 there was printed and sold by the author in Bristol " *An Essay on Mechanical Geometry*, chiefly explanatory of a set of Schemes and Models by which the knowledge of the most useful propositions of Euclid, and other celebrated Geometricians, may be clearly and expeditiously conveyed, even to youth of an early age, by Benjamin Donne, Master of the Mechanics in Ordinary to His Majisty ". Donn and Donne are the same person. To the book is prefaced a motto, " *Quicquid in astronomicis, geographicis, vel nauticis efficiendum ; geometriae attribuendum est.*" The price with the apparatus was two guineas and a half and among the agents for the sale of the book were the famous mathematical instrument makers, Troughton of Fleet Street[1] and Jones of Holborn.[2] The first thing to be noticed is that Donne himself was well aware of the abstract nature of mathematical teaching in his time and said that ;

> " These considerations induced the Author, about thirty years ago, to invent *mechanical*, or *palpable* demonstrations of the most important propositions in Geometry. From 1766 to the present time, he has repeatedly exhibited them in London, Bristol, and other places. In the same way he has exemplified some of their practical uses in Navigation, Architecture, taking of Heights, Mensuration, Gauging and Surveying."

Perhaps we ought to observe next that Donne was a practical man, himself, and published books on Navigation and Surveying ; that he was a mathematical instrument maker ; and that he produced a map of Devonshire which won the first Royal Society of Arts prize for county maps,[3] and which is still considered important by historians of geography.[4] His experience as an itinerant lecturer moreover taught him to express himself clearly and concretely.

The thing that might surprise us most, however, is that Donne's book on Mechanical Geometry was prefaced by a letter from Dr. Beddoes to Benjamin Donne, in which Beddoes says that

> " As all ideas are derived from sense, all argument must consist of a statement of facts or perceptions. The true way therefore of making ideas durable, or rather easily excitable, is to make them distinct at first. It was on this account truly observed, that ' *the art of memory is the art of attention* ". The same end will be answered by any contrivance, calculated to render perceptions vivid. On this principle your tangible proofs of the properties of figures will be eminently serviceable to the intellectual faculties of young people ".

If that were not sufficient to show Beddoes's approval of Donne we have the testimony of J. E. Stock, Beddoes's friend and assistant, that

> " In the month of February, 1796, he [Beddoes] engaged, with his usual activity, in promoting the sale of an Essay, accompanied by and chiefly explanatory of a set of schemes and models of the principal theorems both in plane and solid geometry ; invented by a teacher of mathematics named Donne ; a man well known in Bristol, and respected for unassuming talents and ingenuity. *These models were, of course, particularly acceptable to him, as they practically realized the plan of instruction in that science, which he had sketched out in his Essay on Demonstrative Evidence. . . .* "[5]

[1] Edward Troughton. 1753–1835, see D. N. B.
[2] William Jones, 1763–1831, see D. N. B. under Jones. Thomas.
[3] *Royal Society of Arts Guard Book*, volumes 4, 5, 9 and 10.
[4] G. R. Crone, *Maps and their Makers*, p. 147.
[5] J. E. Stock, *Memoirs of the Life of Thomas Beddoes*, Bristol 1811, p. 128. My italics.

It is precisely that essay which is the basis of Note 2453.

The practical note is observed throughout Donne's volume, in such a way that no pupil could fail to observe the application of geometry to real problems. The following statement is typical ;

> " Having already noticed that the 47th of Euclid's first book (18th in this essay) is looked on as one of the most important propositions in Geometry, it may reasonably be expected that we should shew an instance or two of its usefulness ; we shall therefore give two problems ; one applied to Architecture, the other to Navigation."

Beddoes appreciated the fact that the demonstrations of Euclid were " of use in bestowing a facility in conceiving and recalling long chains of argument." He had no objections to them so long as perceptions were rendered vivid. Donne had spent all his life in exactly that task. His lectures in Bristol, Exeter, Bath, Birmingham and elsewhere were subscription lectures, and to be successful as an itinerant lecturer one had to be able to put over one's ideas in an attractive manner to people who were much concerned with the utility of knowledge. In a note on page 57 he says ;

> " Many years since I invented an easy and mechanic method of conveying general ideas to Ladies and Gentlemen, how it is possible to conduct a ship from one country to another."

Donne, who quotes from Locke's *Essay on Human Understanding*, was not narrow in his mathematical outlook. He did not wish everyone to be a profound mathematician but recognised the importance of mathematics in the training of the mind.

One of the most interesting tributes to Donne's work is the list of subscribers to his volume. It includes S. T. Coleridge,* Francis Burdet, M.P., Erasmus Darwin, Cyril Jackson, Dean of Christ-Church, Oxford, Samuel Galton, Davis Giddy, Benjamin Hobhouse, the Marquis of Lansdowne, William and Joseph Reynolds, the Marquis of Salisbury, Josiah Wedgwood, John and Thomas Wedgwood, William Wilkinson, brother of John Wilkinson, the Rev. David Williams of Knights-Bridge, and Thomas Williams M.P., with many other well-known names of the period. I venture to assert that that subscription list is an epitome of the philosophical movement of the eighteenth century. It includes members of the philosophical societies of Manchester, Birmingham, Derby, Bristol and Liverpool, Fellows of the Royal Society, and of several continental academies, leaders of the cause of constitutional reform in Parliament, in both Houses, engineers, clerics, doctors, industrialists, scientists and poets. The homogeneity of the culture of the age is there represented.

It has been my object to establish the agreement between Dr. Beddoes and Benjamin Donne about the principles of mathematics teaching, and so in the words of his namesake ;

> " . . . having done that Thou haste done,
> I feare no more."

E. Robinson

2604. *Foci of a quadric.*

1. In complex plane geometry, a focus of a conic is usually defined as a point of intersection of two tangents to the conic, one from I and one from J, where I and J are the absolute points (or circular points at infinity). It is then easily shown that if the line equation of the conic is $\Sigma = 0$, and if the degenerate envelopes (other than the point-pair I, J) of the system

$$\Sigma + \lambda(l^2 + m^2) = 0$$

are found, the two pairs of points thus determined are the pairs of foci of the conic.

* Coleridge's address is given as Derby, where he was probably staying with Dr Darwin.

When we consider the analogous case in complex three-dimensional geometry, certain difficulties arise. Firstly, how is a focus to be defined? Secondly, why are foci given by the degenerate envelopes of the system

$$\Sigma + \lambda(l^2 + m^2 + n^2) = 0?$$

A standard method is that of Sommerville (*Analytical Geometry of Three Dimensions*). A focus is defined as a point P such that there is an infinity of triads of planes through P which are perpendicular and conjugate for the quadric (analogous to the property of a focus of a conic that there is an infinity of pairs of lines through P which are perpendicular and conjugate for the conic). He then shows that foci lie on three conics, whose point equations are found, and only later does he show that these conics are the degenerate envelopes of the system

$$\Sigma + \lambda(l^2 + m^2 + n^2) = 0.$$

It is the object of this note to obtain the last result directly. I cannot find this method in any of the standard works.

2. A possible definition of a focus of a conic Σ is that it is the centre of a degenerate circle Φ (pair of lines through the absolute points) which has double contact with Σ. We may define a focus P of a quadric Σ in the same way as the centre of a degenerate sphere Φ (cone through the absolute circle, Ω, or circle at infinity) which has double contact with the quadric. It may be shown that this is equivalent to the definition given above. The definition also implies that Φ has double contact with the tangent cone Γ from P to Σ, so that this tangent cone is circular, that is, a cone of revolution (see Sommerville, pp. 96, 225).

Let the plane equations of P, Σ, Ω be written for brevity as $P=0$, $\Sigma=0$, $\Omega=0$, where P and Σ are respectively linear and quadratic functions of the plane coordinates, and $\Omega \equiv l^2 + m^2 + n^2$. Then Γ is given by $\Sigma=0$ and $P=0$ and Φ by $\Omega=0$ and $P=0$. Since these have double contact, for all planes passing through P (that is, for which $P=0$) we must have $\Sigma + \lambda\Omega \equiv Q^2$, where Q is a linear function of the plane coordinates. Hence, in general, we must have

$$\Sigma + \lambda\Omega \equiv PR + Q^2,$$

where R is a linear function of the plane coordinates. Since $PR + Q^2$ depends on only three linear functions P, Q, R, the equation $PR + Q^2 = 0$ represents a degenerate quadric envelope (conic), and since $P=0$ gives a double root $Q=0$, P is a point of the conic. Hence if we choose λ so that $\Sigma + \lambda\Omega = 0$ degenerates, which we can do in three ways (apart from $\lambda=\infty$, which gives $\Omega=0$), we obtain three conics which together form the complete locus of P.

E. J. F. PRIMROSE

2605. *Note on the number of periods possessed by prime numbers in their recurring decimals.*

When we say that " 7 possesses one period ", we refer to the fact that all proper fractions with denominator 7, when expressed as decimals, are cyclic permutations of one period, 142857. The number 13 possesses two periods in this sense, 11 possesses five periods, and 73 possesses nine periods, which are given in a recent paper by Miss J. S. Batty in the *Mathematical Gazette*. 101 has twenty-five periods, while 239 has no less than thirty-four periods, each of seven digits. Hardy and Wright in *An introduction to the theory of numbers*, p. 114, say " What happens with 7 will happen with any q of which 10 is a primitive root " and add " very little is known about these q " ; q, of course, stands for a prime denominator.

Perhaps the technical language darkens counsel. The aim of this note is to shew that consideration of their form will throw primes into two classes,

according as they possess an odd or an even number of periods. To ask more is to pick out a particular odd or even number and to enquire what primes possess that number of periods. In that respect, 1 is not to be distinguished from other odd numbers.

2. Miss Batty, in the article referred to, furnishes very elegant proofs of the following propositions : (i) the length of the period is the smallest value of x for which $10^x \equiv 1 \pmod{q}$; (ii) if d be this value, then d divides $q-1$; (iii) the quotient $(q-1)/d$ is the number of periods possessed by q. These results can therefore be taken over here. By Fermat's theorem, $10^{q-1} \equiv 1 \pmod{q}$; but $q-1$ is even, and so $10^{(q-1)/2} \equiv \pm 1$. We show that numbers q such that $10^{(q-1)/2} \equiv -1$ have an odd number of periods. 10 is a quadratic non-residue of such numbers, but this is quite irrelevant, apparently.

Assume this congruence ; d is defined above, and let i be an index such that $10^i \equiv m \pmod{q}$. As i takes all integral values from 1 to d, m runs through a series of d residues, no two of which are the same ; for if $10^i \equiv 10^j$, then $10^{i \sim j} \equiv 1$, contrary to the definition of d. These residues are repeated in the same order for $d < i \leqslant 2d$, since $10^{i+d} \equiv m \times 1 \equiv m$, and again for $2d < i \leqslant 3d$, and so on. Finally, $10^{q-1} \equiv 1$ closes the last group of residues, which must therefore contain d members. Hence d divides $q-1$. But we also have $10^{(q-1)/2} \equiv -1$, so that we must have an i, $1 \leqslant i < d$, such that $10^i \equiv -1$. For this value of i, $10^{2i} \equiv 1$, implying that d divides $2i$. But obviously $2i = 2sd$ is inconsistent with $i < d$. If $2i = (2s+1)d$, then *d must be even*. In that case, $10^{d/2} \equiv \pm 1$; $+1$ is ruled out by the definition of d, so that $10^{d/2} \equiv -1 \pmod{q}$.

Let $(q-1)/d = k$. Then $10^{(q-1)/2} = (10^{d/2})^k \equiv (-1)^k \pmod{q}$. Thus $(-1)^k = -1$, showing that k is odd. Conversely, if $k = (q-1)/d$ is odd, $q-1$ being even, d is even. Thus $10^{d/2} \equiv -1 \pmod{q}$ and hence

$$10^{\frac{1}{2}(q-1)/2} = (10^{d/2})^k \equiv (-1)^k \equiv -1.$$

Now $(q-1)/d$ is the number of periods possessed by q ; thus we have shewn that

$$10^{(q-1)/2} \equiv -1 \pmod{q}$$

is the necessary and sufficient condition that q should possess an odd number of periods. If $10^{(q-1)/2} \equiv +1 \pmod{q}$ the number of periods is even and conversely.

3. What are the q's such that $10^{(q-1)/2} \equiv -1 \pmod{q}$? We proceed on the lines of Gauss's Lemma. Consider the residues of

$$10a \pmod{q}, \quad \text{for} \quad a = 1, 2, \ldots, (q-1)/2.$$

Let μ of these be greater than $\frac{1}{2}q$. Then $10^{(q-1)/2} \equiv -1 \pmod{q}$ when and only when μ is odd. Thus μ and $(q-1)/d$ are odd or even together, and one wonders whether there is not some less roundabout connection between the two. However, the residue of $10a$ is greater than $\frac{1}{2}q$ if

$$q/2 < 10a < q, \quad 3q/2 < 10a < 2q, \quad 5q/2 < 10a < 3q,$$
$$7q/2 < 10a < 4q, \quad \text{or} \quad 9q/2 < 10a < 5q.$$

We have to find the number of integers satisfying these inequalities.

If $q = 20r + s$, $|s| < 10$, a lies in one of the intervals

$$r + s/20 \text{ to } 2r + s/10, \quad 3r + 3s/20 \text{ to } 4r + 2s/10,$$
$$5r + 5s/20 \text{ to } 6r + 3s/10, \quad 7r + 7s/20 \text{ to } 8r + 4s/10,$$
$$9r + 9s/20 \text{ to } 10r + 5s/10.$$

If $s = \pm 1$, $\mu = r + r + r + r + r = 5r$;

if $s = \pm 3$, $\mu = r + r + r + r + r = 5r$;

if $s = \pm 7$, $\mu = r + r + r + 1 + r + r = 5r + 1$;

if $s = \pm 9$, $\mu = r + r + r + r + r = 5r$.

If μ is to be odd, we see that r must be an even integer in the expression $20r \pm 7$, and odd in the others. The numbers we are seeking are therefore of the forms $40n \pm 19$, $40n \pm 17$, $40n \pm 7$, $40n \pm 11$. Primes of this form will possess an odd number of periods in their recurring decimals. All other primes will have an even number of periods.

4. The following table contains the earliest of these primes, the number of periods being placed in brackets after them.

$40n+7$	7 (1)	47 (1)	127 (1)	167 (1)	367 (1)	487 (1)	607 (3)
$40n-7$	73 (9)	113 (1)	193 (1)	233 (1)	313 (1)	353 (11)	433 (1)
$40n+11$	11 (5)	131 (1)	211 (7)	251 (5)	331 (3)	491 (1)	
$40n-11$	29 (1)	109 (1)	149 (1)	229 (1)	269 (1)	349 (3)	389 (1)
$40n+17$	17 (1)	97 (1)	137 (17)	257 (1)	337 (1)	457 (3)	
$40n-17$	23 (1)	103 (3)	223 (1)	263 (1)	383 (1)	463 (1)	
$40n+19$	19 (1)	59 (1)	139 (3)	179 (1)	379 (1)	419 (1)	499 (1)
$40n-19$	61 (1)	101 (25)	181 (1)	421 (1)	461 (1)		

The length of the period is first found by finding the value of $\frac{1}{2}d$; the number of periods is deduced by dividing $\frac{1}{2}d$ into $\frac{1}{2}(q-1)$. For example, for $q = 139$, $\frac{1}{2}(q-1) = 69 = 3 \times 23$. We have to build up the congruences of the powers of 10 to 10^3, and 10^{23}. Here $10^{23} \equiv -1 \pmod{139}$. The numbers such as 73, 137, 353 for which $\frac{1}{2}(q-1) = 2^a(2b+1)$ must have $2b+1$ periods, or one only, as for example 113; but such prime numbers seem comparatively rare.

5. Prime numbers of the forms $40n \pm 1$, $40n \pm 3$, $40n \pm 9$, $40n \pm 13$ have an even number of periods. For example, if $q = 239$, $10^7 \equiv 1 \pmod{239}$, $d = 7$ and there are 34 periods. The first few of this class are 3 (2), 13 (2), 31 (2), 37 (6), 41 (8), 43 (2), 53 (4), 67 (2), 71 (2), 79 (6), 83 (2), 89 (2), 107 (2), 151 (2), 157 (4), 163 (2), 173 (4), 191 (2), 199 (2).

C. L. WISEMAN

2606. *Friction.*

In Note 2510 in the *Mathematical Gazette* (May, 1955) it was shown by an example that the force of friction can change its direction suddenly at the onset of motion. Consequently doubt is thrown on the method of solution proposed by Routh (*Analytical Statics*, Vol. I, §§ 181–189) of a certain class of problems. In this note the method will be justified.

The problem concerns a heavy rigid lamina on a rough horizontal table which it touches at points $P_1, P_2, \ldots, P_n$. At each of these points the greatest friction force available in any given direction is given. These data are conveyed by closed convex curves Γ_i surrounding the respective P_i, such that the radius from P_i to Γ_i in any direction measures, in some scale, the greatest friction force available in that direction. If the friction is isotropic Γ_i is a circle with centre P_i. Two points A, B such that $\overline{AB}$ is a unit vector are also given. *It is required to find the magnitude M of the force of greatest magnitude which can be applied to the lamina on the directed line AB without disturbing the equilibrium.*

Suppose that the friction forces, represented by $\overline{P_iF_i}$ in the above mentioned scale, acting at P_i balance a force of magnitude R on the directed line AB. If C is any point not collinear with A and B, the conditions of equilibrium can be written

$$R(C, A, B) + \Sigma(C, P_i, F_i) = 0 \quad \ldots\ldots(1)$$

$$\Sigma(A, P_i, F_i) = 0, \quad \Sigma(B, P_i, F_i) = 0 \quad \ldots\ldots(2)$$

where e.g., (C, A, B) denotes the usual vector product $\overline{CA} \times \overline{AB}$, which is regarded as a pseudo-scalar. (In all summations i runs from 1 to n). The problem is to determine the greatest value M which R, defined as a function of the F_i by (1), can achieve when the F_i vary subject to the constraints (2) and the law of friction:

F_i *is not outside* Γ_i .....(3)

In the first place we solve this maximum problem without regard for the constraints (2). In physical terms we suppose the lamina to be pivoted at C. If $R(C)$ denotes the greatest value of R in this problem we have

$$R(C) \geqq M, \quad \textit{all } C \quad \text{.....(4)}$$

Equation (4) gives an upper bound for M which can be determined easily. If $C \neq P_r$, it is clear that to maximise R, F_r must be put equal to $Q_r(C)$ where (i) $Q_r(C)$ lies on Γ_r, (ii) the tangent to Γ_r at $Q_r(C)$ is parallel to CP_r, (iii) $(C, P_r Q_r(C))$ and (C, A, B) have opposite signs. (These conditions determine $Q_r(C)$ uniquely because Γ_r is a closed convex curve.) It is convenient to define $Q_r(P_r)$ by

$$R(P_r)\overline{AB} + \Sigma\overline{P_rQ_i(P_r)} = 0 \text{ .....(5)}$$

It follows by substitution in (1) that for all C, not on AB,

$$R(C)(C, A, B) + \Sigma(C, P_i, Q_i(C)) = 0. \text{ .....(6)}$$

The next step is to choose C to make $R(C)$ as small as possible so that M is bounded as closely as possible. It turns out that M is determined exactly in this way ! To effect this two formulae are needed. If C, C' are any two points not on AB, it follows from (6) that

$$R(C)(C, A, B) - R(C')(C', A, B) = \Sigma\{(C', P_i, Q_i(C')) - (C, P_i, Q_i(C))\}.$$

If we write $\delta_i = (C', P_i, Q_i(C) - (C', P_i, Q_i(C'))$ we find that

$$(R(C) - R(C'))(C', A, B) = \overline{C'C} \times \{R(C)\overline{AB} + \Sigma\overline{P_iQ_i(C)}\} - \Sigma\delta_i \text{ .....(7)}$$

If neither C nor C' coincides with P_r it follows from the definition of $Q_r(C')$ that δ_r has the same sign as (C', A, B) and is $O(CC'^2)$. If $C = P_r$ we find similarly that

$$(R(P_r) - R(C'))(C', A, B) = \overline{C'P_r} \times \overline{Q_r(P_r)Q_r(C')} + \delta_r - \Sigma\delta_i \text{ .....(8)}$$

The function $R(C)$ is clearly continuous whenever defined, and differentiable except at the points $P_1, P_2, \ldots, P_n$. Consequently it achieves its least value at one point J at least. If $J \neq P_r$, then J is clearly a stationary point and equation (7) shows that $R(J)\overline{AB} + \Sigma\overline{P_iQ_i(J)} = 0$. Hence the $Q_i(J)$ satisfy (2). They already satisfy (3) by definition. If $J = P_r$, (8) shows that $Q_r(P_r)$ and P_r are not separated by any tangent to Γ_r. Hence the $Q_i(P_r)$ satisfy (3). They already satisfy (2) in consequence of (5). The $O(CC'^2)$ terms in (7) and (8) show that J is actually a minimum point of $R(C)$. Hence if $R(C)$ achieves its least value at J, the $Q_i(J)$ satisfy conditions (2) and (3), so that $M \geqq R(J)$. Hence from (4)

$$R(C) \geqq M = R(J) \text{.....(9)}$$

Thus to determine M *is to determine the least value of* $R(C)$.

Finally we show that $R(C)$ takes its least value at not more than one point. For suppose $R(J_1) = R(J_2) = M$. Construct on the " mountain " $R(C)$ a path Π which leads from J_1 to J_2 with the least possible amount of climbing. Let J be the highest point on this path. Then $R(J) > M$ because J_1 is a *minimum* point distinct from J_2. If $J \neq P_r$, then J is a stationary point. For if not a path with less climbing than Π could be found. Therefore $R(J) = M$ which contradicts $R(J) > M$. If $J = P_r$ is the highest point of Π it can be shown from (8) that Π has a definite direction at P_r. Equation (8) also shows that $Q_r(P_r)$ and P_r are not on the same side of each of the two tangents to Γ_r which are parallel to the " horizontal " projection of the direction of Π at P_r. But this is impossible since $Q_r(P_r)$ is a well defined finite point. Thus if

$$R(J_1) = R(J_2) = M,$$

it follows that $J_1 = J_2$.

The conclusion is that the magnitude M of the force of greatest magnitude which can be applied to the lamina on the given line AB is the least value of the function $R(C)$, and this value is achieved by $R(C)$ at exactly one point J. It can be shown easily that the well defined friction forces $\overline{P_iQ_i(J)}$ are the only forces which can balance a force of magnitude M on the line AB. It is clear that the solution obtained agrees with that which would be found by Routh's method in case the friction is isotropic. In particular the point J is the same as Routh's instantaneous centre. It can be seen however that the actual initial motion of the lamina when a force of magnitude greater than M is applied to the lamina on the line AB agrees with that introduced by Routh as a means to his solution for one, but not every possible form of the law of dynamic friction. This special law of dynamic friction has the form: the friction force due to relative motion in the direction $\mathbf{d}$ is $\overline{P_rF_r(\mathbf{d})}$ where $F_r(\mathbf{d})$ is defined uniquely by the conditions (i) $F_r(\mathbf{d})$ is on Γ_r, (ii) the tangent to Γ_r at $F_r(\mathbf{d})$ is orthogonal to $\mathbf{d}$, (iii) $\mathbf{d} \cdot \overline{P_rF_r(\mathbf{d})} \leqq 0$. It is clear that in general the law of dynamic friction need not be related to the law of static friction in this way.

T. A. S. JACKSON

2607. *A cubic.*

It is a truth universally acknowledged by teachers of mathematics that general methods for solving equations are preferable to special dodges.

An example is the solution of the cubic

$$x^3 + 4x^2 + x - 6 = 0$$

by Cardan's method of putting $x = y - \frac{4}{3}$, replacing y by $u + v$ and choosing uv so as to get a quadratic for u. Would anyone care to try this? C. O. T.

[It might be argued that the "truth universally acknowledged" must be modified by the equally universally acknowledged truth that general methods, by virtue of their generality, can not take advantage of special conditions, and that therefore a general method is a last resource, to be used as a routine only when intelligence has failed to find a better method for a particular problem.

T. A. A. B.]

2608. *Two howlers.*

Does any reader know enough about the vagaries of the juvenile mind to explain the mental processes which led to the two following mistakes in a G.C.E. Ordinary Level examination. Both answers were clearly legible.

(i) In finding the difference between two angles whose sines are $\frac{1}{2}$ and $\frac{1}{3}$, first angle 30°, second angle 19° 28′, difference 21° 31′.

(ii) In finding the value of $(a-b)^2$, given $a+b=7$, $ab=10$.
$a=5$ and $b=2$, or $a=2$ and $b=5$.
Thus $(a-b)^2 = 4$ or 559.

The second startled even the hardened examiner. C. O. T.

2609. *Transport problem.*

My daily routine has suggested the following problem:

"I am at a corner where I have the choice of two independent bus systems, buses on each running at ten minute intervals. How long may I expect to wait for a bus?"

The problem of one bus is discussed by J. E. Littlewood in *A mathematician's miscellany* (1953) p. 17.

L. J. MORDELL

REVIEWS

The Polar Aurora. By C. STÖRMER. International Monographs on Radio. Pp. xx, 403. 55 p. 1955. (Geoffrey Cumberlege, Oxford University Press)

Several monographs and articles on the Aurora have appeared in recent years : it is especially gratifying that the latest book on one of the most impressive of natural phenomena should be written by the man who more than any other has enriched the subject—Carl Störmer. As he tells us in the Preface, he began his career as a Pure Mathematician but the chance demonstration by his colleague Kristian Birkeland of the beautiful experiments on cathode rays moving in the presence of a magnetic field changed his whole future. Henceforth his whole interest was directed to the study and observation of the aurora and the author has here collected in book form the whole of his researches on the aurora.

The book is divided into two parts : the first gives an account of the observation, position and classification of aurorae. The second part is an account of Störmer's researches on the motion of a charged particle in the field of a magnetic dipole begun in 1903 and continued almost uninterrupted to the present day. The variety of orbits possible (amply illustrated by the many beautiful plates) is testimony of the great mathematical difficulty of the problem—and also of the infinite patience of the man who pursued this laborious task. It is gratifying that this part of his theoretical researches has found a permanent place in the theory of cosmic rays. Its bearing on auroral theory is less certain though some features of it may be incorporated in an eventual theory. The typography of the book is excellent—as was to be expected of a production from the Clarendon Press.

V. C. A. FERRARO.

Machine Translation of Languages. Edited by W. N. LOCKE and A. D. BOOTH. Pp. vii, 243. 48s. 1955. (John Wiley, New York ; Chapman & Hall)

This is a collection of fourteen papers on the subject of mechanical translation (MT), together with a foreword by Warren Weaver, a historical introduction by the editors, and a bibliography with 46 titles. The central attitude is that illustrated by an extract from a letter that Warren Weaver wrote to Norbert Wiener (quoted in a memorandum which is reprinted in the book) : " When I look at an article in Russian, I say ' This is really written in English, but it has been coded in some strange symbols ' ".

A mathematician will be mainly interested in the possibility of translating by logical formalized procedures. To accept such a possibility is, of course, the first step in any study of MT. It seems that there is general agreement on the fact that a word-by-word translation produces a (more or less) acceptable approximation, that the context should be taken into account (by phrase-by-phrase translation or by more subtle means), that the endings of words play a part which differs from language to language, and that great care is necessary in arranging for a dictionary to be stored on a magnetic drum or some other storage device. The book contains interesting sidelights on idioms and other difficulties and a report on an experiment of MT. There is no agreement on the necessity for pre- and/or post-editing. One author suggests the use of Model English, which aims at the semantic ideal of one-word-one-meaning. It is obvious that MT is easier for scientific texts than for poetry, and translation of mathematical texts should be the easiest of all. (Or is it?)

S. VAJDA.

Theory of Games as a tool for the Moral Philosopher. By R. B. BRAITHWAITE.

Strange things happen at Cambridge. The inaugural lecture by the Knightbridge Professor of Moral Theology, (his chair was originally that of Moral Theology and Casuistical Divinity) deals with the theory of games. However, this is not as absurd as it might appear. A theory of competitive situations must indeed help in making fair decisions when opinions and inclinations clash. Thus the new professor is at liberty to use his knowledge, which he has applied in an earlier book (Scientific Explanation, reviewed in *Math. Gaz.* xxxviii, p. 219), though not to the same purpose.

This time we are invited to consider the following situation: Luke and Matthew live in two adjoining flats which are poorly screened against noise, and they like to play classical piano music and jazz on a trumpet, respectively. They ask our professor to decide how they should act, given their relative preferences of none, one or both of them playing on an evening. He notices at once that this is a two-person non-zero-sum game and proceeds to develop an original idea of how such games should be "solved", without assuming that the utilities of the two players are necessarily comparable. First, the game is solved by a process reminiscent of the classical method; then, advantage is taken of the fact that the interests of the two opponents are not completely contradictory. There is a "cooperators' surplus" which can be shared. The precise form of the division which is here advocated depends, admittedly, on some comparison of the utility scales and is perhaps not quite convincing, but it is proposed with such sweet reasonableness that we are eventually prepared to accept it, since some solution must be found. In any case, the emergence of such a solution is interesting and the mathematical contents of the lecture are deeper than meets the eye. The development proceeds on geometrical lines—using not more than projective properties of the parabola—and an appendix gives, in algebraic terms, the generalization to games where each player has more than two strategies to choose from. At the end, the hope is expressed that in a few hundred years' time branches of moral philosophy "will bask in radiation from a source—theory of games of strategy—whose prototype was kindled round the poker tables of Princeton".

The booklet is so nicely produced, the illustrations are so neat and the style so attractive, that our mathematical, artistic and also—appropriately enough—our philosophical tastes are well catered for.

S. VAJDA.

Cours de Géométrie Infinitésimale. By GASTON JULIA. Troisième Fascicule, Première Partie; Méthodes Générales. Théorie des Courbes. Pp. 220. 3500 fr. 2nd Edition, 1955. (Gauthier-Villars, Paris.)

The first part of the third section of Julia's treatise maintains the leisurely style, the clarity and the thoroughness of exposition which characterized the earlier sections. It contains chapters on the theory of contact, on envelopes, ruled surfaces and line complexes, while the last chapter of 55 pages is devoted to the study of skew-curves, Frénet-Serret formulae and related topics. There is also a chapter on contact transformations which treats the elements of this subject more thoroughly than is usual in elementary texts on differential geometry.

T. J. WILLMORE.

Bericht über den V. Internationalen Astronautischen Kongress. Edited by F. HECHT. Pp. vi, 307. 98s. 6d. 1955. (Springer-Verlag, Vienna).

The International Astronautical Federation was founded in Paris in 1950 with the object of providing a centre through which the interplanetary societies of the various countries of the world could exchange views and

combine to encourage all nations to engage in the research and development necessary to make space flight a reality. In addition, by the organisation of annual congresses, at which papers describing original work were to be presented, it was proposed directly to encourage astronautical research. The 5th Congress took place at Innsbruck in August, 1954 and there it was decided to commence publication of an IAF Journal to be entitled *Astronautica Acta*. This journal was to contain a selection of the papers read at the Congresses and other papers of sufficient merit as these became available. The task of publication was entrusted to Springer-Verlag of Vienna, with Professor F. Hecht acting as editor. Congress papers not included in the new journal were to be published as a separate volume, of which the book under review is the first.

Each paper included in the volume is expressed in one of the languages, English, German, French or Italian. The field embraced by the collection is exceptionally wide and includes discussions of mathematical, scientific, engineering and medical problems. Besides entering into the majority of papers as a theoretical tool, mathematics plays its principal role in the analysis of interplanetary trajectories. In this connection, Preston-Thomas in his paper *Two Aspects of the Time Element in Interplanetary Flight* shows that, over certain periods, physical communication with a planet will become impossible on account of the large fuel expenditure involved in following the only connecting trajectories available. Langton (*The Thermal Dissipation of Meteorites by Bumper Screens*) concludes, as the result of a theoretical investigation, that a thin metal screen surrounding a rocket moving in space would provide sufficient protection against all but the largest, and therefore most infrequent, meteorites. The heat generated by impact would vapourise the large majority of such particles. The suitability for astronautical manoeuvres of many types of circular orbits about the Earth is considered by Ehricke (*Analysis of Orbital Systems*), who also discusses the design of rocket vehicles to ferry supplies into these orbits. Experiments to determine the effect of cosmic rays upon human beings are reported by Eugster (*Demonstration über den Nachweis der kosmischen Strahlung im menschlichen Körper*). Topics dealt with by other authors include propellants, electrical propulsion systems, nomenclature, automatic controls and geodesy.

Although the task of printing technical material taken from so many diverse fields must have been a formidable one, the long experience with this type of work of the publishing house of Springer and their printers has ensured that the result would be difficult to improve upon. It is to be expected that this venture, so well begun, will take on added significance as the years pass

DEREK F. LAWDEN.

Principles of Mathematics. By C. B. ALLENDOERFER and C. O. OAKLEY. Pp. xv, 448. 37s. 6d. 1955 (McGraw-Hill)

The *American Mathematical Monthly* has a deservedly high reputation in this country, particularly for recent articles which explain modern mathematical progress in simple language. Clearly much credit for this must go to the editor, Professor Allendoerfer; and his collaboration with Professor Oakley in the book under review is a further indication of his belief "that large parts of the standard undergraduate curriculum in mathematics are obsolete, and that it is high time that our courses take due advantage of the remarkable advances that have been made in mathematics during the past century".

For a first-year university course, of a very elementary nature, revising algebra and trigonometry and introducing the calculus, to begin with mathematical logic, number-fields, groups, sets and Boolean algebra may seem

Laputian. But no one can maintain that these topics present technical difficulties more serious than those of the calculus, or that their stimulus and outlook value are smaller than for algebra and trigonometry. This strong argument for their inclusion in the curriculum must be subject to the proviso that the inevitable omissions may prune but must not weaken the classical branches.

How does the book stand up to this test? A first reading suggests that the principles of the calculus are clearly expounded, but that the students for whom this book is intended are not expected to acquire any great technical skill until a second course of calculus is undertaken. Trigonometry is rather sketchily handled; the treatment seems too condensed and hurried for the novice and not nearly extensive enough for a revision and development course. The exponential and logarithm are solidly based, but it is surely unfortunate in a newly-planned course to make these functions precede the calculus; this involves the old snag of discussing a^x and then saying that we are particularly interested in the case $a = 2{\cdot}71828\ldots$ though just at the moment we are unable to explain why this extraordinary number should fascinate us. This is not to complain that the authors have done a bad job, but to note that this particular section is naturally difficult to write, and to hope that in a second edition the authors will have had further experience with this section and will write it better.

For the " unusual " sections there can be little but praise. The foundations of logic, groups, abstract algebra are laid clearly, firmly, and at times elegantly. Even the chapter on statistics, which ends the book, is good enough to persuade the sceptic that this subject is not inevitably repulsive. Exercises are reasonably numerous and refreshingly varied; electric circuits provide useful and entertaining applications of Boolean algebra. Each chapter has a short list of references, and, very sensibly, appropriate articles in the *American Mathematical Monthly* are listed. Text-books in England refer to the *Mathematical Gazette* far too seldom.

The whole volume should be carefully studied by those who may be concerned with a broad first-year course in a university, suitable for a general year of work before specialisation. For the Sixth-former the chapters on the " classical " topics would need some alteration, expansion and re-arrangement, but the chapters on the " modern " topics would surely be tremendously fresh and stimulating.

T. A. A. BROADBENT

Mathematics. A Third Course. By M. F. ROSSKOPF, H. D. ATEN and W. D. REEVE. Pp. 438. 26s. 6d. 1955 (McGraw-Hill)

This is the concluding volume of three books on mathematics for use in American High Schools. The first course was devoted almost entirely to Algebra with practically no Geometry. The second volume dealt almost exclusively with Geometry and some Trigonometry. Geometry having been " done ", there is no further reference to the subject in the final course. In the preface, the authors state that their plan was to adopt a more mature approach and to avoid overteaching. The author who " overteaches " tends to teach the teacher and assumes that the latter is devoid of any ideas of his own and is incapable of teaching anything without constant prompting by the author. It is therefore all to the good that the author avoids this fault here. The chief aim of the third volume is the development of the functionality concept and the authors proceed with steady and sure steps with this goal always before them and ever making progress towards it without deviations into side issues and blind alleys. They cover much more ground here than in either of the earlier volumes and the student who completes the course lays a good foundation for more advanced work. The syllabus covered is something between the ordinary and advanced levels of the G.C.E. The authors develop

the functionality concept and give exercises which help to strengthen the pupil's grasp of this notion. Functionality types are considered and built up and these are illustrated by their corresponding graphs. The text deals with the direct and inverse proportion graphs and with linear and quadratic functions and their graphs. The second degree relation is extended to include the graphs of the circle, ellipse and hyperbola as well as the parabola, but these are treated in a very simple way. The treatment is related to the solution of simultaneous equations of the first and second degree. Determinants of the second order are introduced when dealing with the solution of linear equations and these are extended later to third order determinants when dealing with three unknowns. There is an extension of the manipulative work of earlier volumes and the authors assume a greater degree of skill on the part of pupils. There is a chapter on the " number system ". Scales of notation are dealt with in a simple way and the importance of the binary system is emphasized in relation to electronic digital computors and the braille system ; the applications are therefore well up to date. Six of the 17 chapters are devoted to further trigonometry. The general angle and the solution of the general triangle are developed and the formulae used include the half angle formulae and the tangent rule. The addition formulae are given and there is a useful section on fairly easy identities and equations. One chapter is devoted to computation and includes revision of logarithms and slide rules. There is an excellent treatment of approximations and the rules for approximate multiplication and division are the most sensible rules that I have yet seen in a text book.

S. INMAN

Arithmetic operations in digital computers. By R. K. RICHARDS. Pp. iv, 397. 52s. 6d. 1955 (Van Nostrand, New York ; Macmillan)

Electronic digital computers are machines for carrying out a " programme " of arithmetical operations on a set of numbers. In a *serial binary* computer the numbers are transferred from the " store " to the arithmetical unit in the form of a chain of electrical pulses, each pulse being one of two possible kinds, (say) positive or negative, and representing a digit in the expansion of the number in the scale of two. Numbers are added by feeding the corresponding pulse trains into a switching network which emits a third pulse train representing the arithmetical sum of the numbers. Similarly in a *parallel* computer a number is represented by a pattern of pulses appearing *simultaneously* on several lines, the number of lines corresponding to the number of binary digits in the number, 40 being a common choice. In such a computer an adder is a switching network with 80 inputs (40 for the addend and 40 for the augend) and 40 outputs for the sum. This book deals with the logical design of such adding circuits, which are also the basis of the multiplication and division units. Decimal as well as binary computing circuits are dealt with and also various miscellaneous operations connected with computer organisation and control. To the reviewer's knowledge it is the first book dealing specifically with these aspects of digital computers, and is based on a course of instruction given to engineers at the Laboratories of the I.B.M. Corporation.

R. A. BROOKER.

Der Briefwechsel von Johann Bernoulli. I. Pp. 531. Sw. fr. 60. 1955 (Birkhäuser, Basel)

Some fifty years ago the Schweizerische Naturforschende Gesellschaft embarked on the formidable task of publishing the works of Euler ; the Naturforschende Gesellschaft of Basel has now begun a similar undertaking for the great dynasty of the Bernoullis, whose name is writ large over the whole of eighteenth-century mathematics. Some twenty-five volumes will be required, and of these it is thought that ten or twelve will

embrace the correspondence and works of John I (1667–1748). This first volume contains letters written to and by John I up to about 1707 : correspondence with his elder brother James (Jacob), a large and important exchange of letters with l'Hospital, and some miscellaneous letters. The editor, Professor Otto Spiess, has provided a thorough survey of the field in his introduction ; the plan of the undertaking, the collection and discovery of material. Each main group of letters has its own introduction, and there are scholarly bibliographical appendices. Footnotes are judiciously employed, to clarify the text, not in an ostentatious parade of erudition ; many of us, I fancy, might be puzzled by " la fleur de iasmin " (due to Roberval) but the footnote describing this quadrature problem reminds (or informs) us that this is just the loop of the folium of Descartes. The editor has done his work well ; but he has been nobly seconded by the great firm of Birkhäuser. Good paper, clear print in a variety of founts, solid binding, all go to make this volume one of the most attractive ever issued from the famous Basel house.

John I, a prolific and prickly practitioner, rising from medicine to mathematics, shares with Jacob I the credit of being the first to recognise the enormous power of the weapon forged by Newton and Leibniz. These two brothers not only themselves wielded that weapon forcefully and effectively, they inspired and educated Continental mathematicians to grasp the new doctrine, fortunately (on the whole) in the notation of Leibniz rather than through the fluxions of Newton. The collaboration was not smooth, perhaps because it was brotherly ; John was quarrelsome, and not above trying to filch Jacob's ideas. It is therefore not surprising that his claims to have been the creator of much of the doctrine expounded by l'Hospital in his *Analyse des infiniment petits* have often been taken as one more example of his jealous and grasping spirit ; but the letters here printed seem to establish his position as the master, with l'Hospital the pupil, catching and expounding John's ideas and discoveries.

Those interested in the history of mathematics will want these volumes on a readily accessible library shelf. But I hope that young mathematicians may be persuaded to dip into them now and then, for almost every page provides some point of amusement or instruction ; it is fascinating to come here on some master-stroke, there on some clumsiness or blunder showing that even the great can stumble. This is mathematics being made, Vulcan at his forge.

T. A. A. BROADBENT

Theorie der Funktionen einer reellen Veränderlichen. By I. P. NATANSON. Pp. xi, 478. DM 26. 1954 (Akademie-Verlag, Berlin)

This book was first published in 1950 (Moscow, Leningrad) and written for the use of Russian university students. It has now also been introduced, in the present German translation, as an official textbook at the East-German universities. Mathematically this choice can be heartily endorsed. For the author has presented this important but notoriously " heavy " subject of real function theory in a masterly fashion. It must, however, be said at once that he has regrettably seen fit to clothe his account in what can only be called crude nationalistic propaganda. Thus the names of Russian authors indiscriminately appear in fat print (an honour accorded to only the most illustrious foreigners), and curious claims for Russian priority are asserted (but nowhere substantiated). Surely such propaganda is pedagogically undesirable and scientifically unnecessary, since the high importance of the Russian contribution to real function theory, and elsewhere, is nowhere disputed.

Apart from this sad blemish the book is a real pleasure to read. In selection, arrangement, and exposition of the rich material it is an outstanding example of the best continental taste. The author never forgets the student whom he wants to instruct and delight at the same time. He does not indulge in that

severely axiomatic and abstract style that is now so prevalent in Western textbooks. His definitions are always connected with what the reader knows already and he thus justifies them; proofs are not dressed up to record shortness, but their finer points are lovingly explained; no mere (and often cheap) generalizations are proffered that do not apply to the text; and exercises (though often difficult) are genuine and not just further material crammed in. The style is clear and the mathematics rigorous and competent throughout. The student is wisely led to see a very full picture of the whole theory in all its essentials and well into very delicate and up-to-date developments. As I said before, this book gives both invaluable instruction and aesthetic delight, and this is what a good book should do.

It is impossible to do justice to the rich material covered in 17 chapters and on nearly 500 pages of beautiful print, nearly free of misprints. The theory of abstract sets, of Lebesgue measure and integral, of the space L^2, of the Stieltjes integral, of Baire classes, and semi-continuous functions is very fully discussed, and there are excursions into the theory of orthogonal series and functional analysis.

W. W. ROGOSINSKI.

Représentation conforme et transformations àl intégrale de Dirichlet bornée. By JACQUELINE LELONG-FERRAND. Pp. ix, 257. 4000 fr. 1955 (Gauthier-Villars)

This is an original and stimulating book. Mme Lelong-Ferrand considers in the first instance the general class of transformations T from a domain D in the (x, y) plane into Euclidian space of p dimensions, given by

$$\xi_j = \xi_j(x, y), \quad j = 1 \text{ to } p$$

where $\frac{\partial \xi_j}{\partial x}, \frac{\partial \xi_j}{\partial y}$ are continuous and

$$I_D(T) = \iint_D \left\{ \sum_{j=1}^{k} \left[\left(\frac{\partial \xi_j}{\partial x} \right)^2 + \left(\frac{\partial \xi_j}{\partial y} \right)^2 \right] \right\} dx\,dy < +\infty \quad \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

This class is denoted by $\mathscr{D}$. If T is a 1 : 1 conformal map, and $p = 2$, $I_D(T)$ is the area of the image domain. From (1) she is able to deduce a fundamental length area inequality and hence to prove generalisations of many results in conformal mapping. For instance if D is the unit circle, radial and even tangential limits exist almost everywhere. If $p = 2$ and T maps $|z| < 1$ topologically onto a Jordan domain Δ, the mapping remains continuous on the boundary, $|z| = 1$. The Caratheodory theory of prime-ends and their correspondence under 1 : 1 conformal maps of simply-connected domains also goes through for the functions of class $\mathscr{D}$.

In Chapters IV to VI the author confines herself mainly to conformal mappings. She obtains a canonical map of a domain of connectivity p onto a rectangle with $p - 1$ slits parallel to one of the axes by regarding the mapping function as a limit of functions defined on lattice points in the plane, with an increasingly fine mesh. There is also a full discussion of Ostrowsky's condition for a conformal map of D to remain angle-preserving at a boundary point of D and the theory of Ahlfors and the author regarding the existence of an angle derivative at such a boundary point.

These are deep theorems, many of which, although having considerable interest have not previously been included in books. The author deserves our thanks for having made them available to us, in an account which is lucid and gives full proofs. At the beginning of each chapter there is a short summary of the material contained in it and at the end a historical note, referring us to an extremely copious bibliography and index. Here the reviewer felt that the author might profitably have quoted slightly fewer papers and used the space to tell us what some of them contained.

This book is advanced in scope and in many instances brings us right up to the boundary of present knowledge. Perhaps only those who already know something of the more orthodox approach to problems of conformal mapping will be able to appreciate the power and originality of the present author's approach. Her book will be essential for anyone interested in the more theoretical aspects of conformal or quasi-conformal mapping.

W. K. HAYMAN.

Differential and Integral Calculus. By HAROLD M. BACON. Pp. viii, 547. 45s. 1955. (McGraw-Hill)

In this second edition, Professor Bacon of Stanford University has re-written the introduction to limits and the section on differentials, and certain other portions have been clarified. The result is a highly readable and careful treatment of the elements of Calculus. The reader is expected to have some knowledge of co-ordinate geometry, and at a later stage the book includes much work involving solid analytical ; thus at various times polar, cylindrical and spherical co-ordinates are used. The treatment of limits and continuity is sound so far as it goes : some theorems are admittedly not proved, but enough are proved to be suggestive, and in view of the preliminary treatment recommended the student may well be able to deal with $\operatorname{Lt}_{x\to 0} \sin\left(\frac{1}{x}\right)$ and prove that Lt $F[f(x)] = F[\text{Lt } f(x)]$, both of which are given as exercises ; in the latter case the conditions sufficient are well defined. Existence theorems for inverse functions and in other cases are assumed, but a clear statement is always given, and the conditions for the truth of such statements as $\frac{dy}{dx} = 1 \Big/ \frac{dx}{dy}$ are fully stated. The principal values of arcsec, arccosec and arccot differ from those given by Hobson, but have definite advantages. The exponential limit $(1+n^{-1})^n$ being assumed, $D_x \log_a x$ is obtained and $D_x \exp(x)$ deduced ; in all cases where results are assumed reference is made to standard texts, not all of which are American. The existence and value of $D \,.\, x^n$ for n rational or irrational are deduced by use of the exponential function. The work on differentials (first order only) is good and clearly stated. In dealing with maxima and minima it is pointed out that a function may be continuous and have a maximum or minimum point, while its derivative does not possess a constant sign in any interval on either side of the point. The usual assumption of the existence of a maximum and a minimum is made in the proof of Rolle's Theorem ; Taylor and Maclaurin are well treated within the limits imposed by the author.

The fundamental theorem of the integral calculus is related to a certain type of area, which has its own difficulties, but these are pointed out, and the treatment is not unsuccessful. It is refreshing to find " $\int u^{-1} du = \ln |u|$ ", and the chapter on miscellaneous methods of integration is good. Difficulties in evaluating definite integrals by changing the variable are exemplified. Statistical applications of centroid and moment of inertia are mentioned.

There is some good work on partial differentiation : the rule for discrimination of critical points is stated without proof. Iterated integrals are clearly dealt with, but the identification of a double integral as the volume under a surface is less good. A chapter on infinite series quotes many of the properties of convergent series up to Abel's multiplication theorem, but gives no proofs. The integral test for convergence is well dealt with. The last chapter is an introduction to differential equations up to the linear equation with constant coefficients : there is no work on singular solutions or partial equations. A list of standard integrals contains 126 entries. Answers are given to the odd-

numbered examples, and there is an index. There are a few printer's errors, " mutliply " (p. 39, l. 1), " are " (=arc, p. 98, ex. 17), incorrect symbols (p. 88, l. 12 ; p. 120, ex. 25 ; p. 182, l. 5 ; p. 264, l. 3 from bottom ; p. 304: ex. 2).

This book goes up to " S " level plus. With judicious selection it could be used by a science specialist, while the mathematical specialist who used it as a first advanced text-book would gain much from it and would have little to unlearn.

B. A. SWINDEN.

Differentialgeometrie I. By K. STRUBECKER. Pp. 150. DM 4.80. 1955, Sammlung Göschen, Band 1113/1113a. (De Gruyter, Berlin)

This pocket-book contains most of the differential geometry of plane and space curves normally studied in an undergraduate course in mathematics, and in spite of tight packing it remains very readable. The book contains two chapters of which the first is devoted to plane curves and the second deals with space curves. Vector methods are used throughout, the relevant formulae of vector calculus being obtained in the introductory paragraphs of each chapter. There are no exercises by means of which the reader can test his understanding of the text. The printing is good and the illustrations excellent. A second volume dealing with the classical differential geometry of surfaces is in preparation.

T. J. WILLMORE

Equazioni alle derivate parziali di tipo ellittico. By C. MIRANDA. Pp. viii, 222. DM 28.80. 1955, (Springer, Berlin)

With the publication of this work we welcome the revival of Springer's famous series " Ergebnisse der Mathematik und ihrer Grenzgebiete ", which has provided the mathematical world with so many valuable and indeed indispensable surveys of special topics. We have here a survey of methods and results in the general theory of elliptic partial differential equations, covering the period since 1924, the date of Lichtenstein's article on the subject in the Encyklopädie der mathematischen Wissenschaften. The main body of the book is devoted to boundary value problems for second order equations in bounded domains in an m-dimensional Euclidean space ; results for unbounded domains and for equations of higher order are outlined in the final chapter.

The opening chapter sets up machinery for general elliptic equations, analogous to the well known tools for handling the ordinary Laplace equation : maximum properties, general uniqueness theorems, Green's formula, Levi functions (playing the role of the function $1/r$), fundamental solutions, Green's functions. The main boundary value problems (Dirichlet, Neumann, oblique derivative, etc.) are described. The next chapter contains a group of auxiliary results about operator products of singular kernels and various types of generalised potentials ; it is very useful to have these collected here, instead of being scattered, as hitherto, in a multitude of original papers.

Chapter III opens the main attack on boundary value problems for linear elliptic equations. The leading idea is to try to find a solution in the form of a generalised potential with an appropriate Levi function as kernel ; this process leads to an integral equation, to which the Fredholm theory can be applied if the kernel has been properly chosen. The author here shows himself to be a lucid and stimulating guide through the immense mass of papers by Giraud, E. E. Levi, Gevrey and others ; the bibliography lists 53 papers by Giraud alone, several containing more than 100 pages.

Chapter IV discusses methods that become applicable when the equation or the boundary conditions may hold in some generalised sense. Among the topics dealt with here are Wiener's generalised solutions, which lead to an

extension of the theory of subharmonic functions and the Poincaré-Perron approach to the existence theorems, and Weyl's method of orthogonal projection.

Chapter V studies ways of finding upper bounds for a solution of the Dirichlet problem, and for its derivatives and Hölder coefficients, in terms of given boundary values, when it is known only that a solution exists. This line of attack not only makes it possible to show that the solution depends continuously on the boundary values (in Petrovsky's phrase, that the problem is " reasonable "), but also opens up a new approach to the existence theorem ; it is extremely valuable, too, in the treatment of non-linear equations. There is an entertaining proof of the existence theorem, under lighter conditions on the coefficients of the equation than in Chapter III, using a kind of topological continuation of known results about the Laplace equation.

The next chapter deals with non-linear equations, setting out from a general discussion of conditions for the invertibility of non-linear continuous mappings between Banach spaces ; this includes an outline of the Leray-Schauder theory. In the applications to the Dirichlet problem, three types of conditions are required for the validity of the results on solubility : (*a*) conditions on the solubility of the equation of variations or linearised equation, (*b*) conditions on the " reasonableness " of the problem, in the sense described above, and (*c*) conditions on the solubility of the problem for at least one set of boundary values. In certain cases simple sufficient conditions are known to imply a condition of type (*b*), but much work remains to be done.

The book concludes with brief outlines of other recent investigations. In particular, some results have been obtained on the inverse problem : what conditions must an abstract linear functional operator satisfy for it to be representable as an elliptic differential operator?

The book as a whole is an excellent and well organised survey, well up to the traditional " Ergebnisse " standard, of a subject that is still developing rapidly. The author gives clear indications of important unsolved problems and of directions in which new research should go ; little work in this field has been done in Britain, in spite of our long tradition in " applicable mathematics ", and it is clear that there are opportunities here for any who might be interested.

F. SMITHIES.

Mathematical foundations of quantum mechanics. By JOHN VON NEUMANN, translated from the German by R. T. Beyer. Pp. xii, 445. 48s. 1955, (Princeton University Press ; Geoffrey Cumberlege, Oxford University Press)

This work may now almost be regarded as a classic, and it is a pleasure to see an English (or, to be more exact, an American) translation of it. The reviewer wishes this translation had existed when he struggled with the original German about 20 years ago.

The author and the translator have collaborated in producing a text following closely the original German text, and conveying the same meanings.

The book when first published, together with the author's paper in *Mathematische Annalen*, 102, (1929), 49–131, gave the first fundamental contribution to a sound mathematical theory of Quantum mechanics.

R. G. COOKE.

General School Mathematics. Volume I. By P. ABBOTT and E. A. BAGGOTT. Pp. xiv, 282, 31. 7s. 6d. ; with answers 8s. 6d. 1955, (English Universities Press, Ltd.)

This is the first of a series of five volumes planned to cover the ordinary level G. C. E. syllabuses.

We have an opening chapter entitled " Geometry: First Ideas" followed by seven others dealing with factors, fractions and decimals. A return is then

made to geometry and the introduction of literal notation is deferred until Chapter 16. Approximately one fifth of the book is given over to algebraic items, fractions, simple equations and problems being included, but not brackets, Some readers will regret the omission of the introductory chapter on graphs promised for the second volume. The approach is from the particular to the general and is practical whenever possible, as is very evident in the interesting Stage A geometry course. Angle properties of the triangle and polygon are obtained by practical means only, and parallel lines are not discussed beyond the stage of definition and illustration. The emphasis is placed on interest and understanding and many exercises suitable for class discussion are provided. The practical and less formal treatment of the subject, and the limited extent of the ground covered, make this a book which will commend itself, for instance, to those teaching examination and technical streams in Secondary Modern Schools. W. FLEMMING.

Analytic Geometry. By R. R. Middlemiss. 2nd edition. Pp. ix, 310. 28s. 1955, (McGraw-Hill)

This is a second edition but text and examples have been largely rewritten so that the book may be regarded as new. The title would lead most of us to expect a development of geometric properties of figures by algebraic methods, but beyond the formation of the equation of a locus from its geometric definition, little more than curve-sketching is attempted. A good deal of space is devoted to the graphs of given functions. Since differentiation is not used, equations of tangents to circles only are found ; the reflection property of the parabola and the rule for tangents at the origin to an algebraic curve are stated without proof. There is no mention of length of tangent to a circle, and the equation $S_1 + \lambda S_2 = 0$ applied to straight lines or circles appears only as an exercise for the reader.

The author, as he states in his preface, has other purposes which include the importance of verifying the converse of the statement that a given geometric condition leads to some equation of a locus and a clarification of the issue of positive or negative r in polar coordinates. It is a pity that no example of a false converse is given ; the angle in a segment of a circle could provide one. There is a chapter on curve-fitting which includes the use of the method of least squares for the straight line. The book concludes with three chapters on solid geometry, using left-handed axes. Examples are plentiful but restricted in type. C. G. PARADINE.

Mathematics for engineers. I. By W. N. ROSE. 9th edition. Pp. xiv, 527. 21s. 1955, (Chapman & Hall)

Only minor alterations have been made to the 8th edition which has been reprinted many times since 1930. This first part gives a thorough treatment of algebra, plane trigonometry, mensuration and graphs as required by engineers. Harder problems include the calculation of earthwork volumes, plotting of temperature-entropy graphs, construction of alignment charts and other nomograms. There are plenty of easier examples and teachers in schools in search of practical applications of mathematics would find this a useful source-book of engineering formulae and problems. C. G. P.

Functionals of Finite Riemann Surfaces. By M. SCHIFFER and D. C. SPENCER. Pp. x, 451. 63s. 1954. (Princeton University Press ; London : Geoffrey Cumberlege)

A sufficiently able and determined mathematician would profit by reading this book. Such a reader would become familiar with the topology of surfaces, Hilbert space, Green's functions and reproducing kernels, and with variational methods in the theory of functions. The headings of the nine chapters are : (1) Geometrical and physical considerations. (2) Existence theorems for

finite Riemann surfaces. (3) Relations between differentials. (4) Bilinear differentials. (5) Surfaces imbedded in a given surface. (6) Integral operators. (7) Variations of surfaces and of their functionals. (8) Applications of the variational method. (9) Remarks on generalization to higher dimensional Kähler manifolds.

Chapters (4)–(8), which form the main body of the book, no doubt constitute a most important contribution to the development of this part of analysis. The first three chapters are intended to make these accessible to the non-specialist. If the reviewer may judge from his personal experience, they fail to do so. The book deals with a complicated and elaborate mathematical structure, but the natural difficulties are greatly increased by neglect of well established techniques of mathematical writing. Particularly striking is the failure to signpost definitions. These are given in ordinary type, in the body of the text, and without clear beginning or ending. Consider, for example, the "more precise" definition of a Riemann surface on page 25. This occupies one and a half paragraphs, passing without pause into an account of the topology of finite Riemann surfaces. Even more serious is the vagueness of certain crucial definitions.

The reviewer found many proofs difficult to follow. Topological facts are taken for granted. On the other hand, elementary, and easily accessible, properties of Hilbert space are proved at length. F. F. BONSALL.

La Théorie des Fonctions de Bessel (exposée en vue de ses applications à la Physique Mathématique). By G. PETIAU. Pp. 477, 2500 fr. 1955, (Centre National de la Recherche Scientifique, Paris)

Of the four hundred and forty pages of text in this book, three hundred and twelve are devoted to the derivation of mathematical properties of Bessel Functions. Not, it is true, with the rigour found in G. N. Watson's treatise on the subject (this is not the author's intention), for this would have run to more than one large book. However, it would appear that almost all results on Bessel Functions are included here. Bessel Functions of the first, second and third kinds are introduced early and thereafter the book deals mainly with the properties of the general solution of Bessel's equation. The modified equation and its corresponding solutions are discussed at the same time. In the course of the development, the Gamma, Beta, Error, Legendre and Hypergeometric Functions also make their appearance. There is a very interesting, though short, chapter headed "Equations différentielles dont la solution exprime au moyen de fonctions de Bessel".

The part of the book dealing with physical problems begins, appropriately enough, with that of Kepler on elliptic orbits. Chapters follow on the hanging chain, vibrating membranes, heat conduction, diffraction of light and electrical and mechanical oscillations. There is also a chapter on a random walk problem of K. Pearson. In this section of the book, operational methods are often used.

The book is attractively produced; the type is clear and the graphs are beautifully done. The author follows the French tradition of extreme lucidity in presentation; In the first edition of a book of this size, it is perhaps not surprising that there are many misprints; none of those observed are serious, and will not disconcert the reader. The numerous equations and formulae are well numbered, chapter by chapter, but the author omits to make full use of this for purposes of reference. Very frequently, results proved earlier are used without any reference, and the reader can waste a lot of time searching for the result quoted.

A common feature of advanced texts is the omission of exercises at the ends of the chapters. The reviewer feels that this book could have been improved

if some of the results had been omitted from the text and left as exercises (possibly with hints) for the reader.

There is no bibliography, but throughout the book there are copious references to original papers on the topics under discussion.

M. HUTTON.

The Elements of Probability Theory and Some of its Applications. By HARALD CRAMÉR. Pp. 281. 56s. 1955. (Wiley, New York)

The author of this book is professor of mathematical statistics at the University of Stockholm. He is already well known as the author of the standard work *Mathematical Methods of Statistics*, for his contributions to modern probability theory, and for his special interest in actuarial mathematics. The present work is a translation of a modernized version of a Swedish introductory text-book first published in 1926; for English readers it is in effect new.

The book is divided into three main parts. The first, Foundations, establishes the basic mathematical theory of probability and gives a helpful commentary on the historical development of the subject, on the definitions of probability, and on recent attempts to axiomatize the subject. The second part is on Random Variables and Probability Distributions. Here the reader is introduced to the standard distributions (including those of the variables χ^2, t, and F) and to regression and correlation. The third part is devoted to Applications. This includes sections on the computing of sample characteristics, sampling distributions, analysis of variance, quality control, and on statistical inference (which introduces Fisher's maximum likelihood method of estimation and Neymann's concept of the confidence interval). Numerous exercises with answers are given and all the necessary statistical tables are included.

The authority and simplicity of the style of the book are achieved without dogmatism, its clarity and readability without condescension. The conscientious reader will have nothing to unlearn if this introduction leads him to more advanced study of statistics. The argument is illustrated but never obscured by examples; references to further developments are given. The author sets upper and lower bounds to the mathematical attainments of his intended readers and never steps outside them. In short this is a model of what an introductory text-book should be.

The format and printing of the book enhance the high standard already established by the Wiley Publications in Statistics. Unfortunately, however, its high price will prevent its widespread adoption as a class text-book in this country.

B. C. BROOKES.

The Real Projective Plane. By H. S. M. COXETER. 27s. 6d. 1955. (Cambridge University Press)

This book was heartily recommended when it first appeared in an American imprint (*Math. Gazette*, vol. 34, May 1950, page 142), and there is no need to add anything except to say that the author has removed certain minor blemishes of terminology which disturbed at least one reviewer.

It was suggested in the former review that this book should be studied before any systematic study of complex projective geometry is undertaken. Cambridge University could help by setting scholarship questions on real projective geometry, and lecturing on the subject. The geometric interpretation of algebraic equations is not the whole of geometry, and a change of emphasis is long overdue. If it should come, here is the book for the student to read and enjoy.

D. PEDOE.

Calculus. By G. M. MERRIMAN. Pp. xii, 625. 50s. 1955. (Pitman)

The sub-title, " An Introduction to Analysis and a Tool for the Sciences ", indicates the author's aim, which is well achieved, of combining a practical course with a good deal of rigour. Not all the theorems of analysis used are proved, but considerable attention is paid to continuity, mean value theorems, limits and convergence, in a manner which should stimulate thought, especially in a mature student. Such a course should be useful also, as a second reading, to the student who starts the calculus earlier but on a non-rigorous basis.

Nearly half the book is devoted to differentiation, antidifferentiation and integration of algebraic functions, with applications to areas, arcs, volumes and centroids, carefully defined as limits. Differentials are introduced early and the symbol d^{-1} is used instead of the usual integral sign until the fundamental theorem of calculus has been proved. An important step in the logic of this method is the proof, by means of the " function of a function " rule, that the relation $dy = f'(x)\,dx$ is still valid when x is not the independent variable, although in the original definition, $dx = \delta x$ but $dy \neq \delta y$. Integration by substitution becomes a mere operation with differentials, such as

$$d^{-1}\{(x^2+4)^{-\frac{1}{2}}x\,dx\} = d^{-1}(dv) = d^{-1}\{d(v+c)\},$$

where $v = (x^2+4)^{\frac{1}{2}}$.

The second half of the book covers exponential and trigonometric functions, polar coordinates, curvature, convergence and power series, partial differentiation and multiple integrals. Hyperbolic functions are relegated to an appendix. The partial differentiation stops short of harder transformations but is applied to tangent plane and normal and to surface integrals, the necessary elements of solid geometry being first given. Curvature is based on the osculating circle, which is obtained by the use of Rolle's theorem and yields the cartesian formulae for the centre and radius of curvature. In passing, first order differential equations of the types separable, homogeneous and linear, and the second order equation of simple harmonic motion are solved.

Answers are given to some but not the majority of questions. The answer to the dedication, which takes the form of a cryptic definite integral, is perhaps not meant to be easily found by the general reader. C. G. PARADINE

From Simple Numbers to the Calculus. By EGMONT COLERUS. Pp. xi, 255. 12s. 6d. English Edition, 1955. (Heinemann)

The author of this " Mathematics for Everyman " is said to have been self-taught and to have noted his difficulties. At one stage he says " the expert in pure mathematics will probably hold up his hands in horror at our exposition ", and on a first reading this reviewer was inclined to agree with him, but a second reading shewed that the author was not unsuccessful in his object of trying " to combat the general aversion to mathematics " among those who have not had a secondary school education. He is not greatly concerned with practical mathematics, but with concepts ; this influences his order of treatment, so that logarithms do not appear until the penultimate chapter. There are 37 chapters, many consisting of only five pages, and one of only three. There is little attempt at rigidity, and the " philosophy " leaves much to be desired, but the author usually cuts it short with the comment " we must hurry on ". The book covers approximately an Additional Mathematics syllabus ; beginning with three chapters on scales of notation it goes rapidly and lightly through the simpler operations of algebra, Pythagoras, the definitions of trigonometric functions, imaginary and complex numbers, co-ordinates, areas by calculus, lengths of curves, binomial theorem, series, the techniques of differentiation and integration, logs., e^x and interpolation. The treatment is, of course, very elementary and sketchy, but the book has a number of good points and suggestive ideas. Some statements appear strange to us, e.g. the identification of "function " with " equation ". The publishers

imposed a 30 per cent reduction in length on the translators, who admit to having taken some small liberties with the text ; nothing of real importance has been omitted, but an occasional allusion can only refer to the portions expunged. There are at least twenty printing errors, most of which ought to have been corrected in proof.

B. A. SWINDEN.

Plane Trigonometry. By C. R. WYLIE. Pp. viii, 379. 30s. 1955. (McGraw-Hill)

The American educational system frequently results in a somewhat " discrete " course of study in certain subjects, so that the writer of such a book as this is not always sure of the mathematical background of his readers. Professor Wylie compromises : in the body of his text he assumes a good working knowledge of algebra and geometry, but in a lengthy appendix he sets out : the Greek alphabet, common mathematical symbols, the elements of calculations with approximate numbers, " review material from algebra " from the laws of signs up to the binomial theorem, " review material from geometry " quoting practically every elementary theorem of importance, and a glossary of mathematical terms.

The book is well developed from the standpoint that trigonometry is primarily a supporting course for analytical geometry, calculus, physics and engineering, and therefore the analytical aspects are emphasized more than the calculational. The radian is defined at the start, and largely used throughout the book ; and the co-ordinate distance formula is effectively used to prove the general addition formulae and the cosine rule. Elementary derivations are generally well and painstakingly done, though one or two appear a little clumsy. In the work on de Moivre, sine and cosine series, exponential and hyperbolic functions the treatment is formal rather than rigorous, but the pupil is usually warned about points which require more advanced treatment : indeed, the route is well sign-posted throughout with warnings of traps into which the pupil may fall, e.g. the plotting of $x^5 - 5x^3 + 5x$ from the values $x = -2, -1, 0, 1, 2$; intuitive ideas on limits are kept on the right lines, and almost monotonous warnings are given as to the meaning of " $1 \div 0 = \infty$ ". The work on periodicity and inverse functions is admirable, and in view of the declared object of the book there is a good deal of elementary work on vectors. This is limited to the parallelogram law, but the student will learn quite a lot of mechanics. Each topic is related to other topics in the book : there are frequent indications of the future use of trigonometry in other courses, and the author repeatedly indicates that some of the most important applications do not involve angles at all, or only involve them as a matter of convenience.

The book covers about " A " level in this country. It is well printed on good paper (but a symbol is missing on page 22), and apart from a few American spellings and expressions it is written in sober English. But $\mathrm{Sin}^{-1}x$ for the principal value looks strange to us, and the American objection to the bar notation in logarithms seems to make some of the calculations rather clumsy. Page 15 contains some faulty logic. There are 78 pages of tables in which linear interpolation is generally recommended. Answers are given to the odd-numbered examples, and there is a good index.

B. A. SWINDEN

Advanced mathematics for engineers. By H. W. REDDICK and F. H. MILLER. 3rd Edition. Pp. xiv, 548. 52s. 1955. (John Wiley, New York Chapman & Hall)

This popular text has been revised and extended for its third edition. Its main concern is with those differential equations, ordinary and partial, which are most frequently encountered in engineering, though there are subsidiary

chapters on vectors, numerical methods, and complex variable. The standard of rigour is sensible, with references to proofs which have been omitted as too high-brow, though it is a pity that the incorrect statement about Taylor's theorem still disfigures p. 159. There are plenty of reasonably easy examples; a few of more testing character would be welcome. The additions include something about Legendre's functions and polynomials, and a useful extension of the good chapter on Heaviside's calculus and the Bromwich integral by sections on the Laplace transform. A young engineer with a thorough grasp of the differential and integral calculus could use this book with much profit

T. A. A. BROADBENT

Automatic feedback control system synethesis. By J. G. TRUXALL. Pp. xiii, 675. 89s. 6d. 1955. (McGraw-Hill)

Text books on automatic control are appearing so frequently at the present time that, taking into account the fact that the basic theory of the subject is fast becoming standardised, the first point looked for in a new book is the extent to which it differs from existing works. The first conclusion from turning over the pages of this book is that it is not intended for the novice. Indeed the author assumes that his reader will have had a grounding in the subject from some more elementary text, and concerns himself with the detailed problems and techniques of synthesis and design. His method is based throughout on the Laplace transformation, making extensive use of the concept of the transform variable s as a form of complex frequency. Results based on the harmonic response or Fourier transform then fall out naturally as special cases. Transfer functions are defined by the configuration of their poles and zeros in the s-plane. The problems of design are thereby reduced first of all to finding a suitable arrangement of these and then of realising the corresponding operator by a network or equivalent system.

The introductory chapter is mostly mathematics, some of it rather cumbersome. It is followed by a chapter on what is called signal flow. This leads up to a systematic study of the idea of feed-back in its most general form and of the closely related property of stability. After a chapter on network synthesis a full account is given of the root-locus method of W. R. Evans. This is a topic that is fast acquring a permanent place in the text books. It certainly fits naturally into the scheme of the present work. Pole-zero synthesis is the subject of the fifth chapter, while the sixth covers multiple inputs, including noise, and the various methods of deducing the harmonic response locus and hence the transfer function from the step function response. Two chapters are devoted to the problem of optimisation based on statistical methods. Sampling systems are the subject of the next chapter and the book is concluded with two chapters on non-linear systems. The particular methods chosen for detailed consideration in the latter are those using describing functions and the phase plane respectively.

The author has ranged far and wide in the literature of the subject in collecting the material for this soundly written book. It should prove to be a valuable work of reference for the practising engineer.

B. M. BROWN.

Chance and Choice by Cardpack and Chessboard. Vol. II. By LANCELOT HOGBEN. Pp. 490. 70s. 1955. (Parrish)

In this volume Professor Hogben completes his tremendous task of patiently exploring in detail all the assumptions and implications of statistical techniques commonly used in biological research. The first of the two volumes, which appeared in 1950, was reviewed in the *Mathematical Gazette*, XXXV,

312. The second volume broadly covers analysis of variance, regression, factor analysis, and tests of significance.

The sub-title of the book is " An Introduction to Probability in Practice by Visual Aids " and it is as an introduction to statistical method that it must be considered. For what kind of readers then is it intended to be an introduction? The profusion of elaborate black and red diagrams of cards, dominoes, coins and urns, together with the excellent but painstaking exposition of all the elementary mathematics required (including in Vol. I the binomial theorem) suggests that the intended readers are not expected to be well-versed in mathematics or statistics, but may need to use these subjects as ancillaries when their work in medicine or biology yields numerical data to be analysed. Such readers are usually reluctant to devote to an ancillary subject any more effort than is necessary to enable them to grasp the techniques that seem appropriate to their needs and which are commonly accepted in the specialist journals ; they scour the books of recipes for the appropriate formulae, apply them forthwith to their own data, and leave any arguments about the validity of the recipes to the statistical experts who understand them. Professor Hogben rightly deplores this ready acceptance of recipes and disregard of implicit assumptions. In this book his purpose is to expose to the reader all the assumptions and all the mathematical tricks that underly the statistical methods commonly used. In this task he is eminently successful ; everything is taken to pieces and logically displayed in the minutest detail. First the algebra is extracted and explained ; for example, the first chapter of Vol. II is on " The Algorithms of Summarisation " and is an exhaustive analysis of the purely algebraic properties of bivariate populations. Then models are constructed ; thus in the second chapter concrete models of bivariate universes are devised, analysed, and illustrated using packs of cards, balls in urns and so on. Finally the statistical ideas are introduced ; armed now with all the requisite algebraic techniques and understanding of the models, the reader of the third chapter is led to consider the statistical implications of the analysis of variance. The cycle then begins again with chapters on moments and on sampling distributions which lead to a discussion of the significance tests used in the analysis of variance.

Unfortunately, however, for the non-specialist reader, the three chapters leading to the first discussion of the analysis of variance occupy 150 pages of this quarto volume. Certainly, if he works through them the reader will see all the bits and pieces which this minute dissection displays, but it is doubtful whether the reader for whom this book is intended would succeed in putting the bits and pieces together again. The sheer bulk and detail of these chapters (and of the two quarto volumes) is forbidding except to those who already know their way about the subject. Moreover, though one must admire the intellectual honesty of Professor Hogben's final comment on analysis of variance and his last chapter on " Second Thoughts on Significance " in which he admits doubts about the validity of his exposition in the light of recent critical work on statistical inference, this note of doubt is likely only to convince the inexperienced statistician that the intellectual effort Professor Hogben demands of him is more than he can spare for ancillary statistics. Even the conscientious student might well be driven in desperation to the dogmatic books of recipes until the experts have settled their own disputes. This is a miserable reward for Professor Hogben's patience, analytical skill and honesty, but his enthusiasm for his subject has perhaps impaired his estimate of what the non-specialist will stomach.

On the other hand, though the book offers much of interest to him, the reader with more knowledge of statistics or of mathematics will be deterred from exploring it by the bulk of elementary detail that he does not need and which obscures for him the main thread of the arguments. It is suggested that the

reader might skim lightly over some chapters, but it is everywhere difficult to disentangle the argument from its closely interwoven illustrative material, and, moreover, the author develops a private terminology and uses some unconventional symbols (though both terminology and symbols are seen to be rational and convenient when the book is studied as a whole). The reader for whom the book could be of great value is the teacher of elementary statistics, who, if he can afford the outlay of £6 for the two volumes, will find it an inexhaustible source of teaching ideas.

An index is provided in the second volume to cover both volumes. Exercises are set but neither answers nor hints for solutions are given. Some of the visual aids are helpful but many of them are over-elaborate or suffer by the reduction they have to undergo in printing so that their symbols and patterns become difficult to discern (e.g. Figs. 99 and 100). The mathematical printing is excellent in detail and well displayed on the spacious pages ; misprints are rare (though the legends of Figs. 107 and 108 are confused).

B. C. BROOKES.

An Introduction to Stochastic Processes with special reference to Methods and Applications. By M. S. BARTLETT. Pp. xiv, 312. 35s. 1955. (Cambridge, The University Press)

It has long been hoped that Professor Bartlett would write a comprehensive account of stochastic processes, a subject to which he has made notable contributions in recent years. Unlike the treatise by J. L. Doob, the only other comprehensive work on this subject in English, this book is addressed to those more interested in practical applications of the theory than in its underlying mathematics. However, unless the reader already has specialized knowledge of at least some parts of this subject and its mathematical techniques he will find this introductory account difficult. The author covers a wide range of topics in a highly condensed style making frequent references to a companion volume on the basic mathematical theory by J. E. Moyal which, at the time of reviewing, is unfortunately not yet published. As a possible third volume, a systematic discussion of stochastic processes in physics, is also mentioned in the preface, it can be seen that Professor Bartlett's task has been the difficult one of systematizing the topics which have no place in the two as yet unpublished volumes. In effect he has expanded and modernized his mimeographed lecture notes on *Stochastic Processes* which may be known to some readers.

After an introductory chapter on probability theory which establishes his terminology, the author begins with three chapters on random walks, additive and multiplicative chains, with applications to problems of renewal processes, queues, population growth, genetics and epidemics, supported by numerical illustrations and examples. In Chapter 5 he summarizes the differentiation and integration of stochastic variables and briefly describes their several modes of convergence. Then follows a discussion of the harmonic analysis of real and complex processes stationary to the second order (i.e. processes in which the first and second moments of the random variable $X(t)$ are invariant under translation of the t-axis); here the fundamental importance of the " spectral function ", defined in terms of the autocorrelation function of $X(t)$, is demonstrated. Chapter 7 is a summary of Wiener's linear prediction theory for stationary processes and of Shannon's mathematical theory of communication. The last two chapters survey problems of statistical inference arising from the analysis of stochastic processes and the correlation analysis of time-series, subjects of particular interest to the author.

The book thus provides a comprehensive summary of recent work on the applications of stochastic processes and an authoritative review of the

analytical techniques required ; it will therefore interest all who are actively engaged in this field. Engineers, biologists, and others for whom the subject is new, however, will find it difficult to accept the publishers' suggestion that this book provides them with " a self-contained and unified treatment to be read as a whole ".

B. C. BROOKES.

Anschauliche Topologie. By WALTER LIETZMANN. Pp. 172. DM 14.80. 1955. (Oldenbourg, Munich)

The author of this little book has already established a reputation in Germany as an exponent of the popular aspects of mathematics. In this volume he seeks to give effect to Felix Klein's remark, expressed in the geometry section of his *Elementarmathematik vom höheren Standpunkt aus*, on the desirability of an elementary account of topology which is capable of being understood by the mathematical amateur. Lietzmann asserts that what was true in Klein's day is equally true now, but the fact is that topology, in its subject-matter and methodology, has changed profoundly in the last twenty years, particularly in the hands of the algebraic topologist, and therefore this book is in no real sense an introduction to topology. It does not indicate what the living subject is about, nor does it suggest the methods employed by present-day topologists. It is not only combinatorial topology without algebra, it is even combinatorial topology without topology, for the theory discussed is purely combinatorial and no notion of topological equivalence of spaces is given. However, it would be absurd and unfair to discuss the book from this professional standpoint, and for the rest of this review it will be judged primarily as a contribution to mathematical recreation (and, thereby, to general education). As such it deserves better than faint praise, for it makes the fun of this part of mathematics quite evident.

The book is, in fact, a scholarly but readable account of the combinatorial theory of one and two-dimensional polyhedra (or, as we should say, complexes). The first part is called line-topology and is concerned with one-dimensional polyhedra ; the subject-matter here is the theory of threads and the ways of tying them and of linking one or more of them ; knots ; and graphs. This part is quite essentially recreational ; splendid diagrams of knots (in the non-technical sense) abound and the author has rightly decided that quotations from a Seaman's Manual are more appropriate than extensive reference to the work of Alexander, Reidemeister and Seifert.

The second part is called surface-topology, and is concerned with two-dimensional polyhedra ; the subject-matter here is the Euler theorem (for convex polyhedra) ; one-sided surfaces and orientability ; the four-colour problem and other results on contiguous territories ; the projective plane ; and elements of the theory of Riemann surfaces. Of particular interest is the application of the Euler theorem to the study of regular and quasi-regular solids.

The book is especially to be recommended for its excellent diagrams ; its frequent illuminating allusions to the historical development of the subject (thus we are told that Euler's theorem was already known to Archimedes) ; its richness of examples (including the " topology " of chemical bonds) ; its literary asides (limericks on the Möbius band and Klein bottle and discussion of the labyrinth motif in Shakespeare) ; and its references to the literature. Of course, except for the German textbooks recommended for further reading, the literature does not have many coincidences with that of a standard work on combinatorial topology—even Brouwer does not get into the name-index.

A further remark should be addressed to the potential English reader. This book may well cause difficulties of language to one able to read easily a mathematical paper in German. The text is fairly discursive and largely free

from mathematical formalism; moreover, the author deliberately discusses such questions as the connectivity of jackets, trousers and pullovers and ship's cables, and the reviewer certainly found that his previous experience of reading German mathematics had not prepared him adequately for this stiffer linguistic test.

P. J. HILTON.

Praktische Algebra. By H. DÖRRIE. Pp. viii, 259. DM 24. 1955. (R. Oldenbourg, München)

This book represents an extension and generalization of two previous volumes by the same author, one of which deals with quadratic and the other with cubic and quartic equations. The present volume is mainly concerned with the solution of simple polynomial equations of unrestricted degree.

In the early part an account is given of the general theory of polynomials with special reference to the fundamental theorem of algebra, the division algorithm and the properties of symmetric functions. This is followed by a chapter on equations of special type. Later chapters deal with such conventional topics as the upper and lower bounds for the absolute values of the roots, the number of real roots and equations with real roots only.

The numerical calculation of the roots, whether real or complex, is considered in the last chapter, where the general process of iteration is described. Special emphasis is laid, however, on the "Newton-Fourier" method of approximation, which defines the roots by means of sets of nested intervals. The resources of a mathematical laboratory for carrying out the work are not demanded.

A feature of the book is a set of over one hundred miscellaneous examples with solutions, which is included at the end.

The algebra involved throughout the work is neither "abstract" nor "modern" and no reference will be found to systems of simultaneous equations, matrices, groups or the Galois theory.

As its title implies, the book, which is competently written and attractively produced, is essentially practical in outlook. Teachers, computers and technicians alike will find it a useful source of information.

T. A. BROWN

THE MATHEMATICAL ASSOCIATION

Intending members of the Mathematical Association are requested to communicate with one of the Secretaries, Mr. F. W. KELLAWAY, Miss W. COOKE. The subscription to the Association is 21s. per annum and is due on January 1st. Each member receives a copy of *The Mathematical Gazette* and a copy of each new report as it is issued.

Change of Address should be notified to the Membership Secretary, Mr. M. A. PORTER. If copies of the *Gazette* fail to reach a member for lack of such notification, duplicate copies can be supplied only at the published price. If change of address is the result of a change of appointment, the Membership Secretary will be glad to be informed.

Subscriptions should be paid to the Hon. Treasurer of the Mathematical Association.

The address of the Association and of the Hon. Treasurer and Secretaries is Gordon House, 29 Gordon Square, London, W.C. 1.

SHEFFIELD AND DISTRICT BRANCH

Report for the Session 1954–55

Officers elected for the 1954–55 Session were: President, Miss M. J. Meetham; Vice-Presidents: Dr. C. Hamill, Mr. G. W. Wilkinson, Mr. P. J. Wallis; Honorary Treasurer, Miss D. Holdsworth; Honorary Secretary, Mr. C. R. Barwell; Committee members, Miss C. M. Jones, Mr. W. Birkinshaw.

Five meetings were held during the period October 1954 to May 1955.

Wednesday, 13th October, 1954. Miss B. Marchington, of the Royal Naval Scientific Service gave a talk entitled "The preparation of problems for an electronic computor".

Tuesday, 30th November, 1954. We welcomed Professor M. J. Lighthill, of the University of Manchester who spoke on the subject of "Applied Mathematics at School and University".

Wednesday, 13th February, 1955. This meeting was held in conjunction with the University of Sheffield, Institute of Education and was addressed by Mr. K. R. Imeson, Headmaster of Nottingham High School, who talked about "Secondary Modern School Mathematics". This meeting was most successful and was attended by upwards of 200 members and visitors from many of the Secondary Schools in the Sheffield district.

Wednesday, 24th March, 1955. Professor D. G. Northcott, of the University of Sheffield, gave a lecture entitled "Surfaces and Curves".

Tuesday, 11th May, 1955. This meeting took the form of a discussion, "Mathematics for Engineers", in which the opening speakers were Mr. H. Abson, of the Mathematics Department of the University of Sheffield, Dr. D. Harrison, of the Sheffield University Department of Electrical Engineering and Mr. C. V. Gregg, of Huddersfield Technical College.

During the last two years the Branch has arranged two highly successful lectures for VIth form pupils. The first of these was held on Friday, 8th July, 1954 and the speaker was Mr. J. F. Hinsley who spoke on the topic "From examination question to Industrial problem". The 1955 VIth form lecture was given on Friday, 8th July, 1955 and was addressed by Professor T. A. A. Broadbent, of the Royal Naval College, Greenwich, his subject being "Why we have to think about present-day problems in science and technology in mathematical terms and how we do it". These lectures have proved very popular and have been attended by some 300 pupils from the Grammar Schools in the Sheffield area.

The Branch membership has remained steady at about 50 members of whom 29 are members of the Mathematical Association.

C. R. Barwell, *Hon. Sec.*

MATHEMATICAL ASSOCIATION OF VICTORIA

Annual Report 1955

The Association has had another active year during 1955. The Committee met four times, and general meetings were held on seven occasions (including the Annual Meeting). In addition a joint meeting was held in conjunction with the Biometric Society. The programme for the year was:

March. "Setting Examination Papers in Mathematics"—a symposium. Speakers: Mr. O. J. W. Ferres (Scotch College), Mrs. E. S. Hutton (University of Melbourne), Mr. J. L. Griffiths (formerly Headmaster of Dandenong High School).

April. "Mathematics as a Subject of the School Syllabus". Speaker: Dr. K. Cunningham (Australian Council of Educational Research).

June. "Special Leaving Certificate Courses in Mathematics at Melbourne Grammar School". Speakers: Mr. V. B. C. Grenness (Acting Principal), Mr. H. A. H. Smith, Mr. J. Bromley.

June. "A Calculus Peculiarly British". Speaker: Mr. F. J. D. Syer (President of the Mathematical Association of Victoria). (This was a joint meeting with the Biometric Society.)

July. "Mathematical Scrap-book". Speaker: Prof. E. R. Love (University of Melbourne).

August. "Statistics". Speaker: Mr. R. Leslie (Statistics Department, University of Melbourne).

September. "Can a Machine think?" Speaker: Assoc. Prof. F. A. Behrend (University of Melbourne).

October. Annual Meeting and "Engineering Mathematics" demonstrated by students and staff of Melbourne University Engineering School.

A conference of the Australian and New Zealand Association for the Advancement of Science was held in Melbourne during August 1955. Mrs. E. S. Hutton of this Association and Mr. H. M. Finucan of Queensland represented the Australian and New Zealand branches of the Mathematical Association at the Conference.

A committee composed of members of this Association, several members of the Mathematics Standing Committee of the Schools Board, and an inspector of Technical Schools has investigated the supply of mathematics books suitable for school libraries. A list of these books has been sent on behalf of the Mathematical Association of Victoria in conjunction with the Schools Board to all secondary and technical schools in Victoria. Again this Association expresses its appreciation of the interest and valuable co-operation of Associate Professor W. V. Aughterson and Mr. J. Warfe of the Schools Board.

Mr. R. L. Harrison has acted as Recorder this year in place of Mr. H. J. Russell who is a Fulbright scholar in the U.S.A.

Articles and notes by members of the Association have been published during the year in *The Australian Mathematics Teacher.*

F. Roy Manley, *Hon. Sec.*

NORTH STAFFORDSHIRE AND DISTRICT BRANCH

Report for 1955

All meetings during the year, except the Summer Meeting, have been held at Newcastle High School, owing to the kindness of the Headmaster. We have been glad to welcome an increasing number of sixth formers to them.

At the first meeting in February Mr. Diggles, from the Technical College opened a lively discussion on "The Teaching of Mechanics in School". In March, Dr. A. Young of Liverpool University gave an interesting talk on "The present shortage of graduates in Mathematics and Science".

The Summer Meeting was held in May at the University College of North Staffordshire by the kind invitation of Professor Sneddon. Dr. Wallace spoke

on " Modern Geometries ", and then members were provided with tea before dispersing to view the grounds and buildings.

The Autumn activities opened with a discussion in September on " The Teaching of Mathematics in Secondary Modern Schools ", and was introduced by Mr. Johnson.

At the October meeting Dr. Green of the North Staffs. University College spoke on " Mathematical Economics ", and in November, Mr. F. T. M. Smith of the British Ceramic Research Association intrigued us with " Some Unusual Algebras ".

The year closed with the Annual General Meeting and election of Officers.

M. V. GREENWAY

MAKERERE COLLEGE MATHEMATICAL SOCIETY

(Junior Branch of the Mathematical Association)

REPORT FOR 1954–5

The Officers for this session were : *Patron*, Dr. C. P. Welter ; *President*, Mr. J. A. K. Kashangaki ; *Secretary*, Mr. A. J. Lijembe ; *Senior Treasurer*, Mr. I. M. Khabaza ; *Junior Treasurer*, Mr. S. L. Okec ; *Editor*, Mr. F. M. Arscott ; *Committee Members*, Messrs J. W. Namwamba and H. B. Okulo.

Five meetings have been held during the session. At the first, on 13th April, Dr. Welter spoke on " Some Recent Advances in Mathematical Logic ", describing the evolution of Set Theory and the Sentential Calculus. On 18th May Mr. J. L. Joy, of the Faculty of Agriculture, spoke on " The Use of Working analogue Models in Economics " ; his talk was much enjoyed by a good audience who were most interested to see how the flow of liquid through the pipes and tanks of a complicated machine could be used to predict the behaviour of a country's economy. The next meeting was held on 20th July when Mr. I. M. Khabaza took " Infinity " as his subject, devoting his attention mainly to transfinite numbers. The last ordinary meeting on 23rd August was an innovation, as for the first time two student members of the Society, Messrs. J. W. Namwamba and J. P. Mathenge, spoke respectively on " Dimensional Analysis " and " Relativity " ; it is hoped to repeat this experiment later. The session closed with a Social Evening on 2nd October.

The total membership of the Society is 63, and average attendance at meetings has been about 30.

Current copies of the Gazette have been received and circulated, and also the Technical Colleges Report. The Society's own publication, " Ad Absurdum ", which circulates in the College and in a number of Senior Secondary schools in East Africa, has appeared four times during this session.

A. J. LIJEMBE, *Hon. Sec.*

REPORT OF THE CARDIFF BRANCH, 1955–56

OFFICERS : *President*, Dr. R. Morris ; *Treasurer*, Mr. R. A. Jones ; *Secretary*, Mr. W. H. Williams.

Meetings :

Monday, 10th October, 1955 : Miss M. Brooks, the retiring president, introduced a discussion of the optional 0 (e) Mathematics paper of the W.J.E.C. examination for G.C.E.

Monday, 5th December, 1955 : Dr. U. S. Haslam-Jones gave an address : " Some Remarks on the History of Differentiation from 1800 ".

Monday, 6th February, 1956 : The branch was privileged to receive a visit from the President of the Mathematical Association. Mr. G. L. Parsons gave a stimulating and entertaining address on " Mathematics and History ".

Monday, 5th March, 1956 : Professor J. Sears of the University of Capetown gave an address on " A Second Order Differential Equation ", the equation being $y''=f(x)y$. The main problem he discussed was the conditions to be imposed on $f(x)$ for the equation to have no L^2 solution, i.e. a solution $\phi(x)$ such that $\int_0^\infty |\phi(x)|^2 dx$ converges, the integral being a Lebesgue integral.

W. H. WILLIAMS *Hon. Sec.*

EXCHANGE

A graduate Mathematics master in a High School in Victoria, Australia, wishes to exchange with a master in the United Kingdom for the year beginning January, 1957. Any member interested is asked to write as soon as possible for full particulars to :

The Director, League of the British Commonwealth and Empire,
Ord Marshall House, 124, Belgrave Road,
London, S.W. 1.

BOOKS FOR REVIEW

Adams, J. K. *Basic Statistical Concepts.* Pp. 304. 1955. 41s. 6d. (McGraw-Hill Book Company)

Anspach, P. A. L. *Aperçu de la théorie des polygones reguliers.* Pp. 92. 1955. (Brussels)

Arzelies, H. *La Cinematique Relativiste.* Pp. xi, 228. 1955. 2,500 fr. (Gauthier-Villars, Paris)

Banach, S. *Operations Lineaires.* Pp. 249. 1955. (Chelsea Publishing Co., New York)

Bauer, E. *Champs De Vecteurs Et De Tenseurs.* Pp. 204. 1955. (Masson and Co., Paris)

Behnke, H. and Sommer, F. *Theorie Der Analytsichen Funktionen einer Komplexen-Veranderlichen.* Pp. 582. 1955. (Springer, Berlin)

Beth, E. W. *L'Existence En Mathematiques.* Pp. 68. 1956. 900 fr. (Gauthier, Villars, Paris)

Bieberbach, L. *Analytische fortsetzung.* Pp. 168. 1955. (Springer, Berlin)

Blaschke, W. *Einfuhrung in die Geometrie der Waben.* Pp. 108. Sfr. 15.25. 1955. (Birkhauser, Basel)

Bourbaki, N. Livre V. *Espaces vectoriels topologiques.* Pp. 37. 1955. 400 fr. (Hermann, Paris)

Braithwaite, R. B. *Theory of games as a tool for the moral philosopher.* Pp. 76. 6s. 1955. (Cambridge University Press)

Brand, L. *Advanced Calculus.* Pp. xii, 574. 68s. 1955. (John Wiley, New York ; Chapman and Hall)

Brillouin, Leon and Parodi, M. *Propagation Des Ondes Dans Les Milieux Periodiques* Pp. 348. 1956. 4,000 fr. (Masson, Paris)

Brisac, R. *Expose Elementaire Des Principes De La Geometrie Euclidienne.* Pp. 77. 1955. 1,200 fr. (Gauthier-Villars, Paris)

Busemann, H. *The Geometry of Geodesics.* Pp. 402, xiii. $9.00. 1955. (Academic Press Inc., New York)

Bush, R. R. and Mosteller, F. *Stochastic Models For Learning.* Pp. xvi, 365. 72s. 1955. (John Wiley, New York ; Chapman and Hall)

Campbell, R. *Theorie Generale De L'Equation De Mathieu.* Pp. 271. 1955. 2,400 fr (Masson, Paris)

Cartwright, M. L. *The Mathematical Mind.* Pp. 27. 2s. 6d. 1955. (Geoffrey Cumberlege, Oxford University Press)

Centre Belge De Recherches Mathematiques. Colloque Sur Les Questions Des Realite En Geometrie. Pp. 190. 1956. 1,900 fr. (Georges Thone, Liege ; Masson, Paris)

Clapham, C. B. *Arithmetic For Engineers.* Pp. 540. 21s. 1955. (Chapmann and Hall)

Clifford, W. K. *The Common Sense of the Exact Sciences.* Pp. xiii, 249. 1955. (Dover Publications Inc., New York)

Colloque Sur L'Analyse Statistique. Pp. 192. 1,900 fr. 1955. (Masson, Paris)

Coxeter, H. S. M. *Reele Projektive Geometrie Der Ebene.* Translated from the 2nd English edition. Pp. 190. DM 18.60. 1955. (Oldenbourg, Munich)

Damien, R. *Théorème Sur Les Surfaces d'Onde En Optique Géométrique.* Pp. 34. 1955. (Gauthier-Villars, Paris)

Draper, C. S., McKay, W. and Lees, S. *Instrument Engineering III*, 1. Pp. xxviii, 879. 131s. 6d. 1955. (McGraw-Hill)

L'Enseignement Des Mathématiques. Pp. 173. Sw. fr. 12. 1955. (Delachaux et Niestle, Neuchatel)

Enzyklopadie Der Mathematischen Wissenschaften Mit Einschluss Ihrer Anwendungen. Vol. I_1, Part 3_1. Pp. 72. DM. 7.50. 1955. (B. G. Teubner, Leipzig)

Enzyklopadie Der Mathematischen Wissenschaften Mit Einschluss Ihrer Anwendungen) Vol. I_1, Part 7_1. Pp. 26. DM. 6. 1955. (B. G. Teubner, Leipzig)

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